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[THIRD SERIES.]

ART. XLIII.—*An account of the Discoveries in Vermont Geology of the* Rev. AUGUSTUS WING; by JAMES D. DANA.

[Continued from page 347.]

IN the preceding part of this paper the discoveries have been announced of *Trenton fossils* in crystalline limestone in North Castleton, Hubbardton, and Sudbury, within the area of the "great central slate-belt;" in East Cornwall just east of the belt; east of the village of Shoreham; in Eastern Orwell; in Middlebury, and north of East Cornwall—indicating a north-and-south area of Trenton limestone either side and underneath the slate, and showing the slate to be probably the Hudson River shales; of *Chazy fossils* at West Rutland; in East Cornwall; northeast of the village of Orwell; in West Cornwall; in North Cornwall at Ellsworth Ledge; at Weybridge Upper Falls northwest of Middlebury; of *Quebec fossils*, south of the East Cornwall locality of Trenton fossils, and in North Cornwall at Ellsworth Ledge; of *Calciferous fossils* near the borders of West Cornwall at Bascom's Ledge; in Northeastern Shoreham at Mutton Hill, and also in another fold near by (affording a small species of *Orthoceras*, etc.); near the border of West Cornwall at Bascom's Ledge, and at Barbour's Ledge in South Bridport.

The Eolian limestone extends north through Middlebury and New Haven and terminates in Monkton, and affords other fossils in its northern portion.

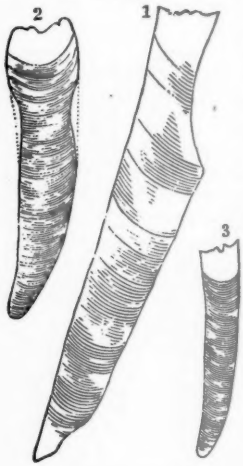
6. *Northern Middlebury, New Haven, Monkton.*

About two miles north of Middlebury, a few rods east of the road running by Messrs. Hammond's to the Severance (or Old

Middlebury) Marble Quarry, half a mile west-by-south from the quarry, across a small stream, there is a thin bed of limestone dipping to the east 70° or 75°, underlaid on the west by six or eight feet of dark fine-grained quartzyte holding *Scolithi.* At this place ten or twelve specimens of a small *Orthoceras* occur, weathered out over the exposed surface of limestone near the place of contact with the quartzyte. The Orthocerata are tapering in form, three to five lines in diameter and one to two inches long, and have very fine close septa. The species is very much like the Calciferous *Orthoceras* figured by Hall in the New York Report.

I visited this locality, on my excursion with Mr. Wing, and found his description right; the specimens are beautifully distinct although only worn sections. Expecting that Mr. Wing would describe the Orthoceras, and also hoping that I should receive from him a specimen (which only long working with a quarryman's tools could safely dig out), I took no satisfactory notes at the time. Through the kindness of Professor H. M. Seely, I have had "squeezes" from three of the specimens, showing well the character of the septa, and have further learned that the number of septa is from ten to thirteen in a quarter inch, that is forty to fifty-two in an inch; and I have also received drawings made on the spot by Miss Parker. The accompanying three figures are from these drawings and the squeezes. They are natural size; and Professor Seely observes that there are indications, though doubtful, that the upper end of No. 1 may have been half an inch longer. The figures are restorations only in having part of the septa more entire than in the specimens.

14–16.



They show that the species is much like *Orthoceras primigenium* Hall, but that part have a slight curve. Mr. Wing's earliest notes on this locality among those in my hands, occur in a letter dated October, 1867.*

Half a mile northwest of the Orthoceras locality and two and a half miles northeast of Middlebury village, and apparently in the same formation with the last, there are specimens resembling *Ophileta compacta;* there was also found here a

* I repeat here that the paragraphs in smaller type consist of remarks by the compiler of this paper, and of facts from other sources.

large *Maclurea*. "The figure here given is correct as to size and general form, though not having the grace of the original."

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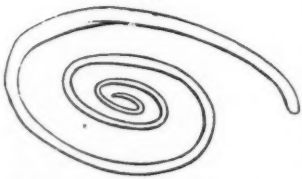

About a mile southwest of the Middlebury quarry and thirty or forty rods west of Mr. E. Kirby's residence, in an old orchard, several distinct convoluted shells were seen on a dark siliceous limestone dipping west. The beds are probably Calciferous.

The quartzyte near the old Middlebury Marble Quarry above-mentioned extends northward into the town of New Haven to New Haven River, a distance of about two miles. The following figure represents a section taken south of New Haven River on the southern border of

18.

Section south of New Haven River on the southern border of New Haven town.

the town of New Haven. There are in succession, going eastward along the lines of section, 150 feet of dolomite (*a*); 200 feet of marble (*b*); 150 feet of dolomite (*c*); 10 feet of gray quartzyte (*d*); 800 feet (*e*) of buff and reddish quartzyte with some slate. The quartzite has been made the overlying rock in the view of Mr. Wing by an overthrow fold. This quartzyte belt, with the limestone west of it, stops at New Haven River. But, 80 to 100 rods to the west, another belt begins which stretches northward, passing just east of New Haven village (four miles from New Haven River), and finally joins the Red Sandrock of Monkton.

At a place two and a half miles south of New Haven village, in a field belonging to Mr. J. Brown, a small *Orthoceras* was found over the weathered surface of the dolomite just west of the quartzyte, which was apparently identical in species with those of the locality above-mentioned. The *Orthoceras* is closely like that of Shoreham (figured on page 342), which was found in limestone adjoining the underlying sandrock. The quartzyte at Mr. J. Brown's, two and a half miles north of New Haven village, is 400 to 500 feet thick, dips eastward at an angle of 50° to 60°, and contains "numerous Fucoids and obscure Scolithi."

This locality is one of those I visited with Mr. Wing. The quartzyte is situated between limestone on the east and west, all dipping alike. The dolomitic limestone to the east has a reddish

color seamed with white, somewhat brecciated, and resembles much the red Winooski limestone (Primordial). The quartzyte ridge was nearly half made up of hydromica slate.

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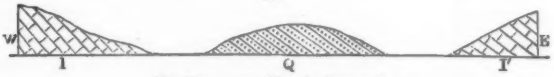

Section near Mr. J. Brown's.

For a mile northeast of the Weybridge Upper Falls, across the railroad, the "striped stratum" is seen in short anticlinals without fossils; but in a fold skirting the western foot of Town Hill, near an old lime-kiln, on the farm of A. Lorraine, three miles southwest of New Haven village, *Rhynchonellæ* were found. The same formation extends on and contains fossils in Brookville, and "in northeastern Middlebury east of Chipman Hill, a little north of Mr. Foot's residence, just east of the road going to Mr. Hammond's."

In the northwestern part of the town of New Haven, one and one-half miles north of New Haven depot, a few rods west of Mr. Charles Mason's residence, the road to Monkton cuts through the western border of a low quartzyte anticlinal, having an eastward and westward dip in the opposite directions. Following the quartzyte northward twenty-five or thirty rods, *Fucoids* and *Scolithi* are found in it. The quartzyte to the west and east dips under heavy beds of "dolomitic siliceous limestone." Farther east there is a thick stratum of white crystalline limestone or marble; next, beds of "sandstone" and concretionary limestone; next, a coarse "sandstone" conglomerate; next, limestone. "Obscure *Orthocerata*, convoluted shells and an *Orthis* were seen," and they appear to be fossils of the "Ophileta beds" or Upper Calciferous. "The limestones end in Monkton not half a mile north of this place," the rock beyond being the Red Sandrock, with its *Scolithi* and *Fucoids*. The Calciferous formation is thus traced to the northern limit of the Eolian limestone.

At my visit to the quartzyte anticlinal just described, I found that there was a small cross-gorge in the quartzyte, showing that the quartzyte had little thickness and that it overlaid a stratum

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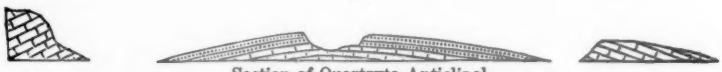
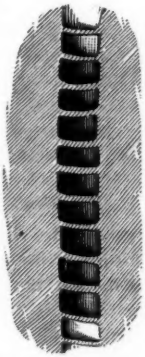


Section of Quartzyte Anticlinal.

of limestone, probably dolomitic, twenty feet in height of which were exposed to view. The beds at this place have only a small dip. The general relations of the beds are shown in the above section.

The quartzyte is in most parts a little slaty in structure, and in limited portions a shining grayish-black slate. In places over it there are areas of sub-concentric conchoidal lamination, looking somewhat as if examples of the flow-and-plunge structure, but more probably a result of concretionary consolidation. To the latter cause I attributed some forms that looked exceedingly like casts of a *Pleurotomaria* and a *Murchisonia*, and of a valve of *Orthis lynx*. Others of these imitative forms over the surface were semi-cylindrical and chambered, as if worn casts of long crinoidal stems; yet having the chambers too large and irregular for any known crinoidal forms. A portion of one of them is here figured natural size; its total length was over ten inches. These simulations of Crinoids may also be due to a concentric structure in the slaty portion of the rock; yet how, it is not easy to understand.

21.



7. *Eastern portion of the Eolian Limestone belt, and the Quartzyte adjoining.*

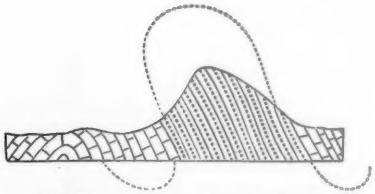
In this eastern portion of the Eolian limestone, constituting one-fourth to one-third of the whole breadth, no fossils have been found. The more crystalline character of the rock accounts for the absence of fossils. Conclusions as to its age and that of the quartzyte have to be based on other considerations. This evidence of intenser metamorphism on going eastward is apparent beyond the limits of the limestone region, to and beyond the Green Mountain axis.

The interstratification of the quartzyte and Eolian limestone is illustrated in the section given on page 340, in which two bands of dolomite are included between belts of the quartzyte.

In July, 1875, Mr. Wing made, along with Professor Seely, a special examination of the quartzyte mountain called the "Hogback," forming the northern terminus of the Vermont Quartzyte range, and lying near the western foot of the Green Mountains. At the north end of the Hogback a low hill, situated north of the road to Starksboro, is of hard grayish quartzyte, while the main range south is brownish and distinctly stratified, with the dip 70° to 75° to the eastward. North of this low quartzyte hill, the quartzyte extends on northward in narrow masses, and is directly overlaid on the northeast by dolomite and bounded by the same dolomite around the whole northern end. The dolomite on the west was not seen in immediate contact with the quartzyte; but at the nearest point, "some rods distant," it seemed to dip east at a high angle, while a few rods to the west it had a decided westward dip. On the *east* side of the quartzyte at this northern end of

the range, and also south to Starksboro, there is another north-and-south range of limestone (dolomite), having an eastward dip conformable to the quartzyte (as represented also in the Vermont Report, p. 346). The figure here given (copied from Mr. Wing's note book) gives his view of the stratification.

22.



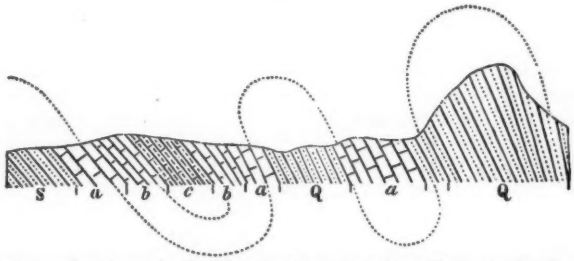
Mr. Wing's section through Hogback.

At Starksboro, *east* of the dolomite, there is a quartzyte ridge which "for a height of 3,000 feet seemed to be an immense flag-stone quarry; the rock is quarried for flagging farther north. It may be called brown quartzyte; it is within the 'Talcose Conglomerate' area of the Vermont Geological Report." The layers are nearly vertical, having a very high eastward dip, conformable with the dolomite and with the quartzyte lying west of the dolomite. The whole valley between the two ranges of quartzyte,—a nearly level strip of country "emoracing two or three villages, excellent farms and farm-houses,"—seems to have been made by erosion, the dolomitic and slaty strata having been worn away faster than the quartzyte of Hogback on the west and the Flag-stone ridge on the east. The only fossils of the quartzyte are the *Scolithi* and *Fucoids* observed elsewhere, and these are the same with those of the Red Sand-rock.

The conclusion given in the notes of this excursion in 1875 is,—that "all the rocks in Bristol north and east of Mr. Oaks's residence (on the road next west of Bristol village), all in Monkton and in Starksboro, are older than the Calciferous; the dolomites belong to the Red Sandrock series, or that of the quartzyte, and in some places appear to be 400 to 500 feet thick; and they overlie the quartzyte, being beneath because of an overturn anticlinal." These dolomites here referred to the Red Sandrock series are those called "Subcrystalline limestone" on page 343, which contain the Calciferous *Orthoceras* at Mutton Hill and are there referred to the Lower Calciferous. The following section (from the letter of 1872,) represents the rocks between the Red Sand-rock west of New Haven (see map) and the quartzyte at the village of Bristol, and Mr. Wing's view as to the folds. *a*, The Red Sand-rock dolomite or "Subcrystalline limestone;" *b*, The "Ophileta beds" or Calciferous; *c*, The "Conglomerate" or "Trilobite bed" or "siliceous limestone greatly resembling it," underlying to the south on the strike, "Rhynchonella beds" containing fossils;

Q, quartzyte. Adjoining the Red Sandrock on the west is No. 2 of the section on page 343, a "dolomitic sandstone."

23.



Mr. Wing's section from the Red Sand-rock west of New Haven to the Quartzyte at Bristol village.

In the excursion with Mr. Wing we passed along by the western foot of the Hogback range, north of the village of Bristol. At one point the limestone stratum was seen to form the lower part of the quartzyte bluff, and to dip beneath the quartzyte at a small angle, as if actually an underlying stratum. Mr. Wing spoke of this limestone at the time as part of the Primordial or Red Sand-rock series.

A section of Hogback in the Vermont Geological Report, taken along a line south of the region examined by Mr. Wing, between Bristol and Lincoln, makes the limestone at the *west* base

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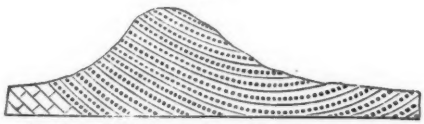


Section of Hogback, from Vermont Report.

of Hogback dip eastward 45° or so beneath the quartzyte. To the eastward of Hogback along this section no limestone is represented in the figure; but the Report says (p. 346): "in the valley of the north branch of New Haven River, passing up to Starksboro, there are ledges of limestone occasionally seen, interstratified with the quartz rock, nearly as far as Starksboro village; and there is reason to believe that it may extend to meet a narrow belt of limestone running south from Hinesburgh."

In my examination of the limestone and quartzyte ridge northeast of Rutland, I found that the dip in both was eastward; but the nearest outcrops of the two were too remote to make it sure that they were conformable. They are conformable according to the section through Rutland and Mendon given in the Vermont Geological Report. The quartzyte in Vermont is in many places interstratified with, and replaced by, hydromica slate (sometimes

chloritic), or a hydromicaceous quartzyte or conglomerate—a fact dwelt upon in the Vermont Geological Report, which says as follows, when describing sections across the eastern quartzyte range. In Sunderland the quartz-formation includes, with quartzyte, talcose schist [that is, hydromica slate] (p. 614). In Wallingford the quartzyte and quartz conglomerate are interstratified with talcose schist (p. 627). "Talcose schist is associated with the quartz-rock of Mendon" (p. 634). In Goshen "the quartz-rock formation is composed of hyaline quartz, talcose schist, and argillo-talcose schist" (p. 640). In Ripton "the quartz-rock is composed of ten bands of different rocks, viz: hyaline quartz, compact sandstone, talcose and chlorite schist" (p. 645). Speaking of the belt of "talcose conglomerate" it says that it includes sandstones, breccias, quartzyte, coarse conglomerates, talcose schist, novaculite schist, and "talcose schist is the most common rock in the belt" (pp. 386, 387).

I have examined this association of hydromica slate and quartzyte in the quartzyte ridge northeast of Rutland (the ridge making the western boundary of the town of Mendon). Having on my second visit struck the ridge at a different point from that examined on my first visit, I was perplexed by finding hydromica slate in place of true quartzyte; but afterward reached an explanation on observing the gradations of one rock into the other. The Geological Chart of the Vermont Report does not generally represent this interstratification of the quartz and hydromica slate, as the Report observes, because the details were not separately made out owing to the intimate relations of the two. It is shown, however, in Section VIII, where, near Ripton, occurs the remark "Quartz rock interstratified with talcose schist;" and bands of color in the colored section represent the fact.

8. "*Great Fault of Western Vermont.*"

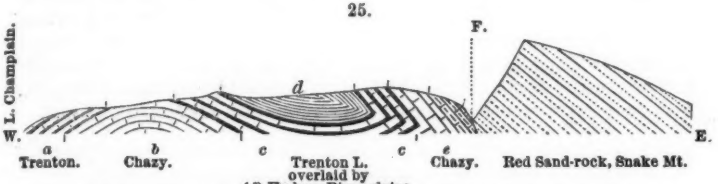
Snake Mountain is a north-and-south ridge of Red Sand-rock several miles long, situated to the west of Middlebury, within seven miles of Lake Champlain. It has a steep western face, and gradual eastern slopes—the former across the stratification, and the latter nearly conforming with it. This western side is the course of a great fault.

The rock of Snake Mountain east of the fault is the Red Sand-rock of the Vermont Report, and this continues to be the surface rock eastward to Otter Creek. At the southern extremity of the mountain, in Bridport, west of the mountain, there are the successive Lower Silurian formations, but in an inverted position. The following section gives the order and position of the beds observed at this place.

The first and uppermost rock west of the fault containing fossils is the Chazy; below this comes the Trenton, and next the Hudson River, so that the Chazy and Trenton have been folded

back for two or three miles upon the slate. This arrangement in the main prevails along the west front of Snake and Buck Mountains to near Vergennes.

25.



Mr. Wing's section in Bridport, across south end of Snake Mountain.

"Along the highest part of Snake Mountain no slate or limestone is seen at all. At the north end of the mountain the Red Sand-rock has been lifted until it is 700 feet above the level of the Hudson River slates to the west, while at the south end, four or five miles distant, it is much less elevated. Around its northwest end, only the Hudson River slate is seen. Then the Chazy comes in behind the Trenton, and both are folded over on the slate. The Chazy, Trenton, and the Hudson River slate disappear in the same order farther south. The Red Sand-rock (Potsdam) appears to have been pushed up and thrust west over these other formations; and so far over, opposite the highest part of the mountain, as to conceal first the Chazy, the Trenton and the Hudson River beds; then, farther north, leaving the Hudson River beds, Trenton and Chazy, along the lower parts of the mountain."

The Chazy beds on the west of the fault afford large *Maclureæ*, one or two species of *Orthoceras*, an *Orthis;* the Black River limestone, *Columnaria alveolata* in great masses, besides other species; the Trenton, *Trinucleus concentricus*, and various other fossils, and the Hudson River slate its characteristic species.

This fault continues south through Bridport, Eastern Shoreham and Orwell to Orwell village and beyond, "but with some irregularity of direction, it following neither a meridian nor the line of a belt or fold."

"Two or three miles south, half a mile west of the village of Shoreham, and south by Sisson's Hill and Barnum's Hill, in the south of Shoreham, the Potsdam proper is folded over on to the Hudson River slate." "Six miles south, 1½ miles west of the valley of Orwell, at Chittenden's Mills, in a deep gorge made by the stream, the Chazy, holding large *Maclureæ*, is seen folded over the Hudson River slate, etc." Mutton Hill adjoins the fault. Again in Waltham, eight or ten miles north of Bridport, the Red Sand-rock is brought up against the Trenton.

Prof. Emmons describes the fault at Snake Mountain and gives a section in his American Geology (vol. I, Part 2, p. 87, 1855); but his section seems to have been taken north of the point described by Mr. Wing; the place is not stated. It makes the formations to the west of the fault lie in regular instead of inverted order. Professor Emmons supposed the rock of Snake Mountain, raised by the faulting, to belong to his Taconic System and therefore pre-Silurian. Prof. Hitchcock, in the Vermont Geological Report, gives a section which represents the mountain *without the fault.*

9. *Conclusions of Mr. Wing as to the Geology of the part of Central and Southern Vermont investigated by him.*

1. The hydromica slates, clay slates, Eolian limestone and quartzyte, with the so-called talcose conglomerate on the east, are all of Lower Silurian age, and conformable in superposition.

2. The Eolian limestone is not Taconic, as made by Prof. Emmons; nor of some one formation, as implied in statements in the Vermont Report and in the name it gave it; nor of the Quebec group, as inferred by Logan; it includes Lower Silurian limestones of various periods, the Upper Potsdam or Lower Calciferous, Calciferous, Quebec, Chazy, Black River, and Trenton.

3. The Red Sand-rock on the west of the Eolian limestone—admitted to be Potsdam or Primordial in age—and the Quartzyte on the east which often rises into mountain ridges, are of the same formation, and come nearly or quite together in Monkton, on the northern limits of the limestone area.

4. These rocks—the Red Sand-rock and Quartzyte—are the western and eastern borders of a great abraded synclinal, the axis of which in its northern part has a slight northward rise (or southward dip), the sides there coming together; and which also has both the eastern and western sides of the fold eastwardly inclined, the dip of the beds being generally eastward. Between its borders there are, in some parts, subordinate longitudinal abraded anticlinals and synclinals.

5. The slates of the "great central belt" are of the age of the Hudson River slates (or that of the Cincinnati group), for the reasons stated on page 345—which are briefly these: (1) the slate-belt is bordered in several places on both the east and west sides by limestone affording Trenton fossils (*Trinucleus*, etc.), and no where else have Trenton fossils been found in the Eolian limestone; (2) there are also narrow north-and-south outcrops of limestones containing Trenton fossils over the interior of the slate-belt, as if brought up from beneath by anticlinals; (3) the slate-belt is plainly underlaid by the limestone at its north end; (4) in Whiting the Trenton limestone of the Sudbury area has a direct connection, across the slate area, with the limestone of Otter Creek valley, east of the belt, which also is Trenton in age —the slate being interrupted "for forty or fifty rods."

The Trenton limestone has been identified west of or within the "central slate-belt," at localities but a few miles apart, in all the towns north of Castleton (the most southern on the map illustrating this paper, p. 335), including, in succession, Hubbardton, Sudbury, Whiting, Shoreham, Cornwall, Weybridge; and east of the slate-belt, in Leicester, Eastern Cornwall and Middlebury. The Chazy limestone adjoins the "central slate-belt" in West Rutland.

6. The several Lower Silurian limestone formations lie in north-and-south belts, with the lowest—the Potsdam and Calciferous—nearest to the Red Sand-rock, or to the Quartzyte, and the upper—the Trenton or Chazy—nearest to the "central slate-belt." This view is presented in the section from East Shoreham to East Liecester on p. 340.

This is confirmed as regards the Trenton limestone, "Sparry limestone," by the list of its localities just mentioned. The Chazy ("Rhynchonella beds") so well represented by fossils at West Rutland, has afforded fossils also in Leicester, East Cornwall, Middlebury, and the same fossils have been found on the east, as on the west, of the "central slate-belt;" while the "striped stratum," which is a marked feature of the Chazy,—"a way-mark by which the rock can be recognized without its fossils," occurs also in Middlebury, in the western part of Brandon by the Burgess Field farm and along the quarries of this part of the town, in the western part of Pittsford, and in Northern Salisbury; and "doubtless it reaches the marble quarries of West Rutland, and continues south through Tinmouth and the whole State; and in this formation is embraced the Birdseye and Black River limestones." "On Crown Point, Lake Champlain, these 'striped beds' are largely developed at the old Forts, and are perhaps 100 yards beneath the Black River limestone—whether in the Chazy or Birdseye is not known. There is a stratum of seemingly pure sandstone, resembling the Potsdam, overlying them at Crown Point. These facts may help to solve the age of the same 'striped beds' in Addison County."

The groups affording the lowest Calciferous fossils on the west of the "central slate-belt" occur next to sandstones of the Red Sandstone formation; and also at two localities north of Middlebury, adjoining beds of quartzyte, or where the limestone and quartzyte come together. The "Ophileta beds," or those referred to the Upper Calciferous, come next, being more remote from the sandstone or quartzyte, as found to be true in Shoreham, Western Cornwall, Weybridge, Middlebury and New Haven.

The beds occur, with their fossils, in Salisbury, Leicester and Brandon.

The eastern range of limestone, or that forming the eastern margin of the great Eolian belt, a mile in width in many places

but narrower to the north, is concluded to be Calciferous or Upper Potsdam, because it adjoins the quartzyte, and is dolomitic, the limestone nearest to the outcrops of quartzyte in Northern Middlebury and New Haven being proved by its *Orthocerata* to be—like that nearest the Red Sand-rock to the west of Shoreham—of Calciferous age, if not Upper Potsdam.

The localities affording the Calciferous fossils (Ophileta beds) in Salisbury, Leicester and Brandon lie a little to the west of the great marble formation; and this marble formation is considered the equivalent of the western "Subcrystalline limestone" just above the fucoidal sandstones or Upper Potsdam. The dolomites farther east belong to the Upper Red Sand-rock series, or else the bottom of the Calciferous, as has been elsewhere stated.

The later formations extend less far north than the older because of the inclined axis of the great abraded synclinal: "the Hudson River slates (those of the "central slate-belt") reaching central Weybridge; the Trenton, about a mile farther; the Rhynchonella beds five or six miles farther north; and finally these disappear, owing to the rising into view of the Red Sand-rock."

7. The quartzyte of the eastern range, with that also of the local belts in the Eolian limestone area, is regarded as Potsdam (or Primordial) in age, because it contains in many places *Scolithi* (worm-burrows) and *Fucoids* like those found in the Potsdam sandstone; because also it adjoins Calciferous limestone beds at the localities just mentioned in North Middlebury and New Haven; and because it joins the Red Sand-rock in Monkton, and one rock has in many places the character of the other, although not commonly alike in color, and showing differences explainable on the ground of the greater metamorphism of the quartzyte. "In Monkton, the Red Sand-rock and the Quartzyte meet in a succession of short anticlinals, thus cutting off to the north the great trough or synclinal;" and "the Red Sand-rock absolutely overlies the beds of Red Sand-rock in one anticlinal and the quartzyte in another anticlinal, and both hold *Scolithus linearis.*

8. The quartzyte belt of Rutland Center (just north of Rutland), with the associated slate, extends north by Sutherland Falls nearly to Forestdale, and it runs south of Rutland Center into Clarendon and Tinmouth. It lies throughout within the Eolian limestone area. But it is seen on the map that the area is on the line of strike of the quartzyte-range north of Pittsford. This quartzyte is regarded as Potsdam; and the limestone which lies directly to the west of it (half way from Rutland to the West Rutland valley) is supposed to be possibly the "Ophileta beds" or Upper Calciferous, and the "Conglomerate" or Quebec

group. "But this narrow valley is a very disturbed region, and the limestone seems to be greatly compressed between the quartzyte belt on the east and the slate belt on the west (separating it from West Rutland valley)." "The region was studied farther south in the valley as well as to the north, to ascertain what rocks occurred, and the conclusion was that nearly all the formations found in other places here occur; that is, the older on the east against the quartzyte (No. 1), and then the others in succession, with the Trenton against the slate bounding the limestone on the west, while the slate is No. 8 or the Hudson River slate." No fossils were found in it.

10. *Historical Note.*

The preceding notes have been taken chiefly from the letter from Mr. Wing to me dated August 9, 1872. They show that his view, that there are Hudson River slates in the Eolian limestone region, antedates my own; for my paper on the subject was not published until 1873, and there had been no communication between us before then on the subject, or on any subject. Moreover, my own views as to the age of the Berkshire rocks were based chiefly, as I stated, on Mr. Wing's discoveries in Vermont. I cite the following from a letter to me, of May 8, 1875, written soon after he had first seen my paper on the subject—a letter never, however, received by me until his papers were recently put into my hands. It shows that his conclusions date as far back as 1866.

"When, in 1866, the *Trinucleus concentricus* and other Trenton fossils were found first in Sudbury, underlying on the west side the great central mass of slate running south from Weybridge through the State, embracing the "Talcoid schists" capping Dorset and Manchester Mountains, Mount Anthony in Bennington, and also Graylock in Massachusetts, I reached the conclusion at once that all these slates in Southwestern Vermont and Southwestern New England were neither of the Quebec group, nor Taconic, but of the age of the Hudson River slate. In the progress of my investigations, I assumed, and I often expressed in correspondence with Mr. Billings, that the 'Eolian limestone' of the Vermont Geological Report embraced not only the Trenton and Hudson River beds, but all the formations of the Lower Silurian as well, and even limestones and dolomites of the Red Sand-rock series. I was wrong at first in assuming that these same slates overlying the Trenton and Chazy limestones along this central belt extended also west to the Hudson River and were all of the same age. For the Primordial fossils recently collected at Bald Mountain, Washington Co., and east of Troy, New York (both of which places I have visited), have modified my view as to the western extent of the Hudson River slates. But they have not weakened my belief in their existence in Southwestern Vermont and New England."

From the preceding account of Mr. Wing's discoveries it is evident that he performed well the task he laid down for himself in 1865—the determination of the age of the Eolian limestone. Knowing that fossils were the only sure criterion of geological age, he searched, and he found them, and thus reached sure conclusions. For the western portion of the Eolian limestone and more than half the eastern (that of Otter Creek Valley), the special geological age was thus determined, and the several Lower Silurian formations identified. He further made a right use of the facts, when, in view of the Trenton and Chazy age of the fossils in limestone along the borders of and within the "central slate-belt," and the observation that the beds more remote are successively older—he deduced that the slates were younger than the limestones holding the Trenton fossils, and therefore, in all probability, of the Hudson River (or Cincinnati) group, and that they lay in a synclinal with Trenton and older limestones beneath and on either side.

The more highly metamorphic condition of the limestones making the eastern border of the Eolian limestone prevented his giving to the geology of this part of the region the same positive basis from fossils which he had obtained for the rest.

The Quartzyte also proved almost barren, yielding him only *Scolithi* and *Fucoids*, neither of which serve to fix positively the age of the beds. His argument with respect to it from the well-defined Calciferous fossils found adjoining quartzyte at three different localities on the west and north is however a strong one, and seems to set the question at rest for those outcrops. The Vermont Geological Report states that the eastern Quartzyte range, near Rockville in Starksboro, has afforded a *Lingula*—the specimen containing "scores of fossils but none very distinct"—which Prof. James Hall "regards as a new species related to one in the Medina sandstone," and as evidence "though unsatisfactory" that the quartzyte is of the age of the Medina. From the same Quartzyte range farther south, in the vicinity of Lake Dunmore, Prof. C. B. Adams, as it states, found a shell near a *Modiolopsis* in form; also a tapering shell looking like an *Orthoceras*. Yet the determinations of all these fossils are admitted to be doubtful, and the question of age is still an open one.

One of the most important points established by Mr. Wing is the conformability of the Lower Silurian formations throughout the region. From the Red Sand-rock, or Primordial, upward they make one consecutive series, and all are involved, as Mr. Wing urges, in one system of synclinals and anticlinals. The quartzyte, hydromica slates, and limestones, associated on the eastern border of the region, and the great bands of limestone, hydromica slate and clay slate (or roofing slate), with some quartzyte, making the center and western portion, are of one system, and took, together, their present positions. The great fault which made Snake Mountain was simply one of the breaks and displacements attending the mountain-making movement, as shown years since by

Logan. The observations of Mr. Wing afford nothing to sustain the view that there was an epoch of disturbance in the region at the close of the Primordial or Cambrian period, but, on the contrary, they prove that the rocks went on forming in regular succession nearly or quite to the close of the Lower Silurian; and that then followed, as Mr. Wing concludes, the epoch of upturning and metamorphism. The making of the Green Mountains has for many years been referred by some geologists to this epoch, on the basis of the fossils in the limestones of Vermont. These fuller developments leave no doubt that this view is right, at least so far as the Eolian limestone of Vermont and the associated schists and quartzyte are concerned.

In another number of this Journal I will close this subject by stating the bearing of the Vermont facts on the geology of Berkshire.

ART. XLIV.—*On Barite crystals from the Last Chance Mine, Morgan County, Missouri; and on Göthite from Adair County, Missouri;* by G. C. BROADHEAD.

1. *Barite from Morgan County, Missouri.*

THE rocks of the barite locality in Morgan County, Missouri, are of the age of the Second Magnesian Limestone. A shaft has been sunk in a spring through masses of tumbled rock displaying what seemed, in Missouri miners' parlance, to be a "circle" of about twenty feet diameter. This "circle" was found to be filled with fractured masses of limestone, sandstone and clay for forty feet in depth, or to the bottom of the shaft when I visited it. These masses of rock were often found studded over with beautiful crystals of barite. The galenite was also often covered with such crystals. In some cases a thin coating of transparent barite covered the rock to which the crystals adhered, but they were often seen loosely adhering to the naked rock.

The form of these crystals is different from any heretofore found in Missouri. They are elongated into prismatic forms ($\frac{1}{2}$-$\bar{2}$) in the direction of the macrodiagonal axis and are attached to the rock by one of its extremities. The development of the basal plane gives them a flattened appearance. The free extremity of the crystals is gradually sharpened off to a thin edge through the presence of very low macrodome. This is so irregularly developed, however, as not to admit of trustworthy measurements, but seems to have the symbol $\frac{1}{3}$-$\bar{2}$. The central portion of all the crystals toward the attached extremity is perfectly transparent. This clear portion has a rhombic form corresponding to the fundamental (cleavage) prism, and is

very sharply defined, the remainder of the crystals being of an opaque milky white.

There being a spring of water in the mine. the water may be sometimes charged with an excess of mineral solutions. The lead has probably been deposited from an aqueous solution and the barite from similar and more recent solutions. The waters also seem to have subsequently held iron oxide in solution as some of the crystals of barite are coated with it.

A crystal in my possession from the Cordray mines, but probably from same mine as the above, possesses similar characteristics. It is of rhombic form, about two inches long and a half inch thick; the edges are bevelled by the plains $\frac{1}{2}$-$\bar{i}$ which gave on measurement the angle 77° 40′. It has a border a half inch wide of white opaque barite, the outer angle sharp, inner angle truncated, and the remainder is clear mineral, which corresponds in shape to the cleavage prism; the direction of the cleavage is also indicated by cracks across the crystal. The white portion of this crystal would seem to indicate alteration, but that can hardly be the cause of the similar appearance of the crystals from the Last Chance mine.

2. *On the Göthite from Adair County, Missouri.*

The Coal measures, it is well known, often contain, within the thicker shale beds, interstratified beds of clay ironstone, sometimes in connected layers, at other times in concretionary masses, occurring along a marked horizon. These concretions are often reticulated by calcite veins, one system in concentric lines, the other crossing them; they are generally termed septaria.

In 1873, while examining the structure of the formations on Chariton River, Adair County, Missouri, I broke open a number of the concretionary masses which were reticulated by veins of calcite from a quarter to half an inch wide, occasionally widening into small cavities one-eighth to one-half an inch high, lined with calcite crystals reposing on the more compact calcite of the vein. Upon and sometimes interpenetrating these crystals are small slender acicular crystals of black göthite, generally not more than an eighth of an inch, but sometimes as much as three-fourths of an inch, in length. The göthite is generally aggregated in bundles at the base of the calcite from which they radiate. Sometimes a single crystal will shoot directly through a calcite crystal. Sometimes also, a single point of a quartz crystal will be seen penetrated in various directions by the göthite. The calcite, often, has its angles bevelled or rounded, and they were sometimes colored red on the surface as was also a little of the göthite. Minute velvety balls of göthite not larger than the one-thirty-second of an inch were also found reposing on the calcite.

Pleasant Hill, Mo., March, 1877.

ART. XLV.—*Estimation of Chromium and Aluminium in Steel and Iron;* by ANDREW A. BLAIR.

HAVING had occasion, several years since, to examine a number of samples of so-called "Chrome-steel," for the percentage of chromium, I began by searching for this element in the residue left after acting on the steel with dilute HCl, as, in the case of cast-iron, this plan is recommended.* Failing to find it here, except in very small amounts, there seemed to remain two methods of procedure: the first being to fuse the sample with Na_2CO_3 and KNO_3, either directly† or after oxidizing‡ by HNO_3 and evaporating to dryness; the second, to treat the solution of the steel containing the iron as a ferric salt, with KHO, Na_2CO_3 or Na_2CO_3, and $NaC_2H_3O_2$, in excess, and in this alkaline or acetic acid solution oxidizing the Cr_2O_3 to CrO_3, by means of Br, Cl, PbO_2 or $KMnO_4$, which combines with the alkali as a soluble alkaline chromate, while in the cold or upon boiling, the iron is precipitated as hydrated sesquioxide or basic acetate.§ While both of these general methods will undoubtedly give good results, when large amounts of chromium are present, so that small amounts of the original substance can be operated upon, they present many objectionable features when the percentage of chromium is small, and the amount of steel or iron that must necessarily be used is correspondingly large. The principal objections are, in the first method, loss by spirting during fusion, and in both, the difficulty of washing the voluminous precipitate of iron free from alkaline chromate. A third general method, viz: separating the Cr_2O_3 from the Fe_2O_3 by means of $C_4H_6O_6$, NH_4HO, and NH_4HS, exactly as in the separation of Al_2O_3 and Fe_2O_3, might be added, but the difficulty of washing such a mass of ferrous sulphide seemed quite impracticable, and the method was not even attempted.

By means of barium carbonate, Cr_2O_3 may be perfectly precipitated,‖ and thus separated from the great mass of the iron, which in a hydrochloric acid solution of the steel would exist as a ferrous salt. Following out this plan the result was the following method.

Five grams of borings or drillings are weighed out into a flask

* Wöhler's Mineral Analysis (Nason), p. 204.

† Watts's Dict. of Chemistry, vol. iii, p. 374.

‡ Rose's Chim. Anal. Quant., (French ed., 1862), p. 987.

§ Crookes's Select Methods, p. 127. W. Gibbs in Am. Journ. Science (II), xxxix, 58. Chancel, Compt. Rendus, xliii, 927.

‖ Rose, Chim. Anal. Quant., p. 513. Fresenius, Quant. Chem. Anal., vi ed., English, p. 379.

of about one-half liter capacity, twenty cubic centimeters strong HCl diluted with three to four times its volume of water poured in, and the flask closed with a rubber stopper, provided with a valve, such as is used in dissolving iron wire in volumetric analysis. Heat is applied as required, and, when all the steel is dissolved, a solid stopper is quickly substituted for the one with the valve, and the flask and contents cooled. When cold, the solution is diluted with cold water until the flask is about three-fourths full, and a slight excess of $BaCO_3$ added with constant agitation. The $BaCO_3$ should be free from $BaSO_4$, as it obscures the reaction, and too great an excess should be avoided.* The flask should be thoroughly shaken several times, loosening the stopper occasionally to allow the CO_2 to escape, and allowed to stand over night. It is then filtered as rapidly as possible, the flask rinsed out several times with cold water, and the precipitate on the filter (consisting of all the Cr_2O_3, the residue from the steel insoluble in dilute HCl, some Fe_2O_3 and the excess of $BaCO_3$) washed well with cold water. The filter is then punched and the precipitate washed into a small clean beaker, the portion adhering to the sides of the flask dissolved in HCl, which is poured on the filter and the filter cleared from any Fe_2O_3 by HCl and thorough washing with hot water.† If sufficient HCl has not been used in clearing out flask and washing filter, enough is now added to dissolve all the Fe_2O_3, Cr_2O_3, $BaCO_3$ in the beaker, the solution diluted, boiled and the Fe_2O_3 and Cr_2O_3 precipitated by NH_4HO, the boiling being continued until all smell of NH_4HO has disappeared. This precipitate is filtered and washed thoroughly with hot water to get rid of all $BaCl_2$; dried and transferred to a platinum crucible, carefully separating the filter, which is ignited and the ashes added to the precipitate (which should not be heated). A mixture of three grams Na_2CO_3 and one-half gram KNO_3 is now added, and the whole fused carefully, the heat being raised gradually until all the KNO_3 is decomposed. After cooling, the fused mass is treated with hot water, and the soluble portion containing the Cr as alkaline chromate, with the excess of the alkalies, separated from the Fe_2O_3 by filtration, the precipitate and filter being thoroughly washed with hot water. The filtrate is acidulated with HCl, and evaporated to

* It is almost impossible to buy $BaCO_3$ free from $BaSO_4$, the very best brands containing from 5–30 p. c. of $BaSO_4$. I prepare my own by dissolving $BaCl_2$ in water, filtering, adding large excess of NH_4HO and passing CO_2 into solution, until all the barium is precipitated as $BaCO_3$, washing thoroughly, drying, and grinding in water to the consistency of cream.

† If it is desired to determine the chromium soluble in dilute HCl alone, or separately from that which remains in the insoluble residue, the filter should not be punched, but the Fe_2O_3, Cr_2O_3, and $BaCO_3$, dissolved on the filter, in dilute HCl, and a separate determination made of the Cr in the insoluble residue (which remains on the filter) by burning the filter and fusing with Na_2CO_3 and KNO_3.

dryness with a little alcohol; when thoroughly dry, the Cr_2O_3 is dissolved in HCl, diluted and filtered from silica, the Cr_2O_3 precipitated by NH_4HO, filtered with the usual precautions, ignited and weighed as Cr_2O_3,* calculated to Cr by the factor 0·6853. Atomic weight of Cr 52·2.

The only impurity this precipitate can contain is a little Al_2O_3, partly from the Al in the steel, and partly as an impurity in the Na_2CO_3 and KNO_3. The best method for separating this impurity, is to add to the solution of alkaline chromate obtained above (after fusion and filtration from Fe_2O_3) an excess of $KClO_3$, then a slight excess of HCl, and evaporate to syrupy consistency on the water bath, adding a little $KClO_3$ from time to time, so that there may always be an excess to decompose any HCl. Redissolve in water, add a slight excess of $(NH_4)_2CO_3$, and boil off all smell of the latter, filter, wash with hot water, add to solution an excess of HCl and, after the greater part of the $KClO_3$ is decomposed, a little alcohol, and precipitate the Cr_2O_3 as before.† In Genth's method (loc. cit.) of evaporating the solution of alkaline chromate nearly to dryness on the water bath, with an excess of $(NH_4)NO_3$, a minute amount of CrO_3 is reduced to Cr_2O_3, and precipitated; it is consequently filtered off with the Al_2O_3, SiO_2, etc., and lost, unless this residue should be treated separately to regain this Cr_2O_3, which very much complicates the process. If in the filtrate so obtained the CrO_3 is precipitated by $HgNO_3$ as Hg_2O, Hg_2CrO_4, some of the chromium salt nearly always passes into solution upon filtering, and can be detected, sometimes in very considerable amounts, in the filtrate. The same thing occurs if this method of precipitation is used, after neutralizing the solution of alkaline chromate (obtained by fusion with Na_2CO_3 and KNO_3, and filtering from the Fe_2O_3) by HNO_3. In all cases I have obtained the most satisfactory results by reducing the CrO_3 to Cr_2O_3, precipitating by NH_4HO, and weighing as Cr_2O_3.

This method for estimating chromium in steel may appear a little tedious, but quite a number can be made at once and only about three to four days is required for a complete estimation. The accuracy is all that can be desired and the delicacy of the method in detecting and estimating very small amounts, greater than that of any method I have used. I think I can safely say that it is delicate to 0·005 per cent of Cr. The amount of Fe_2O_3 usually precipitated by the $BaCO_3$, with the Cr_2O_3 is about 0·05 gram.

For techinal work the method can be very much shortened by fusing the precipitate by $BaCO_3$, with Na_2CO_3 and KNO_3 or

* For precautions necessary to free Cr_2O_3 from alkaline salts, see Genth in Chem. News, vi, 30. Fres. Chem. Anal. Quant., 387.

† Dexter's method, see Rose, Chim. Anal. Quant., p. 520. Fresenius, Chem. Anal. Quant., p. 372. Pogg. Anal., lxxxix, 142.

$KClO_3$ (when $KClO_3$ is used instead of KNO_3 the precipitate must be thoroughly ground in and incorporated with the flux before fusing) dissolving in water, filtering, and determining the CrO_3 volumetrically. The results obtained in this way are fairly good, but are apt to be a little low.

Aluminium, nearly always exists as such in steel, and may be estimated with great accuracy by proceeding exactly as in the above method for the determination of chromium, until after filtering and washing the precipitate by $BaCO_3$, which is then dissolved on the filter in dilute HCl and the solution allowed to run into a small clean beaker. This solution is diluted, boiled, and the barium precipitated by a slight excess of dilute H_2SO_4; the $BaSO_4$ allowed to settle, filtered, washed, and the filtrate evaporated nearly to dryness to get rid of the excess of acid. This solution is then diluted, and the Fe_2O_3, and Al_2O_3 separated by $C_4H_6O_6$, NH_4HO, and NH_4HS. If the steel contains any chromium it will be with the Al_2O_3, and must be separated by fusing the residue obtained by running to dryness the filtrate from the FeS and igniting with Na_2CO_3, and KNO_3; dissolving in water and, without filtering, adding $KClO_3$ and HCl, as before in the separation of Al_2O_3, and Cr_2O_3. The Al_2O_3 obtained on precipitating by $(NH_4)_2CO_3$ will be contaminated by small amounts of SiO_2, and CaO (from the $C_4H_6O_6$) from which it can be separated by dissolving on the filter in HCl, after washing free from alkaline chromate, into a small clean beaker, running to dryness to render SiO_2 insoluble, dissolving in HCl, filtering, and precipitating the Al_2O_3 by NH_4HO, being careful to boil off all smell of NH_4HO. After filtering and careful washing the precipitate can be dried, ignited, and weighed as Al_2O_3, calculating to aluminium by the factor 0·5331. Atomic weight, 27·4.

The great solubility of the Cr of chromium steel in the most dilute HCl certainly seems to indicate the existence of a true alloy of iron and chromium, and that a large part at least of the chromium exists as such, and not as an oxide or in any intermingled slag. Mr. E. Riley,* at a meeting of the Chemical Society, March 15, 1877, presented some specimens of chromium pig-iron containing from six to seven per cent of Cr. He mentioned the fact, during his remarks, that the Cr had dissolved with the Fe in the course of analysis. The president, Professor Abel, F.R.S., said that he had examined a specimen of the so-called chromium steel, but had found a mere trace of chromium in it. It was possible however, he said, that the chromium exerted a function in the production of the steel, but was eliminated at some stage in the process, so that it did not appear in the finished steel.

* Chemical News, No. 904, March 23, 1877.

In this connection the following analyses may not prove uninteresting, showing as they do that, in some cases at least, the chromium does "appear in the finished steel."

Sulphur,	0·005 per cent.	trace	trace
Phosphorus,	0·021 "	0·020 per cent.	0·005 per cent.
Silicon,	0·129 "	0·189 "	0·279 "
Total carbon,	0·825 "	0·935 "	1·199 "
Comb. carbon,	0·811 "	0·920 "	1·186 "
Graphitic carbon,	0·014 "	0·015 "	0·013 "
Manganese,	0·245 "	0·062 "	0·039 "
Copper,	0·007 "	0·010 "	0·005 "
Nickel,	trace	0·023 "	0·021 "
Cobalt,	0·016 "	trace	0·018 "
Aluminium,	0·034 "	0·029 "	0·026 "
* Chromium,	0·651 "	0·223 "	0·916 "
Chromium, soluble,	0·615 "	0·212 "	0·899 "
Chromium in residue,	0·021 "	trace	trace
Slag,	0·320 "		

The determinations of Cr marked * were made by the first method without the separation of Al, and consequently are a little higher than the sum of the "chromium soluble in dilute HCl," and "chromium in residue," which were determined in the portion and by the method taken for estimation of Al.

The "slag" in the first analysis given above, was determined by the "iodine" method and contained by analysis Fe_2O_3 0·110 per cent, Cr_2O_3 0·160 per cent, undetermined 0·50 per cent, total 0·320 per cent.

Laboratory of U. S. Board appointed to test Iron, Steel, etc.
Watertown Arsenal, Mass., April 14, 1877.

ART. XLVI.—*On the Chemical Composition of Triphylite, from Grafton, New Hampshire;* by SAMUEL L. PENFIELD. *Contributions from the Sheffield Laboratory of Yale College.* No. XLVII.

THE rare mineral species, triphylite, is found at Grafton in a granite vein which has been extensively worked for mica. It occurs imbedded in the granite in masses of varying size, and occasionally of fifty or more pounds in weight. The exterior of the masses in some instances shows evidences of the decomposition to which the mineral is peculiarly liable, but a large portion of the material is fresh and unaltered. It has a light blue color, a greasy to resinous luster and a perfect cleavage. Specific gravity =3·52. Analyses of the finely pulverized, pure mineral, dried over sulphuric acid, show the following composition:

	I.	II.
P_2O_5	44·18	43·88
FeO	26·09	26·38
MnO	18·17	18·24
CaO	·89	·99
MgO	·56	·61
Li_2O	8·77	8·81
K_2O	·32	·32
Na_2O	·16	·09
H_2O	1·47	1·47
	100·61	100·79

The phosphoric acid was separated from the iron and manganese by fusion with sodium carbonate, except a small amount retained with the iron, which was separated afterward by means of molybdic acid.

The alkalies were separated from the iron and phosphoric acid by adding a small amount of ferric chloride to a nearly neutral, oxidized solution of the mineral and precipitating the iron and phosphoric acid together by means of barium carbonate. Care was taken to use platinum vessels in conducting the evaporations. The total amount of water was expelled by ignition and determined by absorption in a chloride of calcium tube. Water amounting to 0·50 per cent was driven off at 100° C. These results show the Grafton mineral to be richer in manganese and lithia than that from Bodenmais.

The relative number of atoms calculated from analysis 1 are the following:

P	·622					6·22
Fe	·362					
Mn	·256	·648				6·48
Ca	·016					
Mg	·014					
Li_2	·292		Li	·584		
K_2	·003	·297	K	·006	·594	
Na_2	·002		Na	·004		
		·945				

The above gives the ratio of the phosphorus to the bases $= ·622 : ·945$ or $1 : 1·52$, which shows the composition of the mineral to be that of a normal phosphate. The ratio of the atoms of the univalent elements to those of the bivalent is $·594 : ·648 = 1 : 1·09$ which gives the formula $10\overset{I}{R}_3PO_4 + 11\overset{II}{R}_3P_2O_8$. Rammelsberg * deduces the formula $4\overset{I}{R}_3PO_4 + 5\overset{II}{R}_3P_2O_8$ from the most trustworthy analysis of the Bodenmais mineral, but suggests $\overset{I}{R}_3PO_4 + \overset{II}{R}_3P_2O_8$ as possibly the true formula. The

* Mineralchemie, 1875, page 307.

composition here obtained for the Grafton triphylite renders it almost certain that this is the correct formula of the mineral. This work was conducted in the Sheffield Laboratory under the supervision of Professor O. D. Allen, to whom I wish here to express my thanks.

ART. XLVII.—*On a new Mode of Manipulating Hydric Sulphide;* by JOSIAH P. COOKE, Jr., Erving Professor of Chemistry and Mineralogy in Harvard College.*

IN chemical laboratories, where instruction in qualitative analysis is given to large numbers on the class system, the use of hydric sulphide gas as a reagent is attended with grave inconveniences. These evils can in great measure be avoided by substituting for the gas a solution of the reagent in water, saturated at the ordinary temperature and pressure of the air; when, as is well known, one volume of water dissolves about 3·4 volumes of the gas (measured at 15° C. and seventy-six centimeters). Such a solution was for a long time used in the laboratory of Harvard College. It was prepared in a long series of two liter bottles connected by glass tubes in the usual way, and the solution was kept in the laboratory in a large tubulated glass flask, from which it was drawn by the students, as occasion required. This solution answers almost every purpose for which the reagent is used in the ordinary course of qualitative analysis, and the few conditions under which it does not give satisfactory results can be easily avoided. Moreover, in ease of application and promptness of effect it has all the advantages of a liquid reagent; and the only inconveniences its use involves is an occasional evaporation of a solution, which the dilution by the reagent may render necessary. Of course a solution of hydric sulphide is liable to oxidation, and soon becomes turbid in contact with the air; but this change can be easily avoided by fitting to the neck of the flask (in which the solution is kept), by means of a rubber stopper, a glass tube dipping under the liquid, and connecting this tube with one of the vents of illuminating gas in the laboratory.

For quantitative work, and for the preparation of chemical products, when considerable quantities of metallic sulphides must be precipitated, a solution of hydric sulphide, saturated under the ordinary pressure of the air, is inconveniently dilute; and two years since we described a simple method by which a solution concentrated under pressure could easily be prepared with the ordinary laboratory appliances. A heavy glass bottle

* From the Proceedings of the American Academy, vol. xii (N. S. iv), p. 113, 1877.

of about four liters' capacity served as a generator, and from this the gas passed through a wash-bottle into two other bottles of the same size and strength as the first. The intermediate bottle was three-fourths filled with water, while the last served simply as a gasholder. The connections were so arranged that the gas, after bubbling up through the water, was delivered at the top of the gasholder; and, by a tube uniting the bottom of the gasholder with the water-pipes of the laboratory, the interior of the apparatus could be submitted to the pressure of a column of water sixty feet high. The gasholder was also provided with an exit tube, which could be closed by a compression cock. The connecting tubes all passed through rubber stoppers which were firmly wired to the necks of the bottles, and the water-bottle was connected with its neighbors by two lengths of stout rubber hose so that the water could be shaken up with the gas without disturbing the rest of the apparatus. In order to saturate the water with hydric sulphide, a charge of ferrous sulphide, sulphuric acid and water sufficient to yield at least three times the amount of gas theoretically required was placed in the generator, and, after the connections were made, the gas was allowed to steam through the apparatus until all the air was displaced. The exit tube of the last bottle was then closed, the water pressure turned on and the water-bottle frequently shaken, until absorption ceased, and the aqueduct water—at first forced by its pressure into the gasholder—was driven back into the pipes. Before dismounting the apparatus, the rubber connectors were all closed by compression cocks, and care was taken to vent the generator gradually. Moreover, the connectors of the water-bottle were so arranged that when the apparatus was dismounted a short piece of rubber hose was left attached to each orifice of the bottle, both of which were closed by compression cocks. To one of these a vent tube was subsequently attached, and by this the solution was drawn off at pleasure, as from a soda-water siphon.

This simple apparatus was constantly used by us for two years, and served an excellent purpose; but it was found that after the glass generators had been charged several times they were liable to burst under the same pressure which at first they had readily sustained. The same accident never happened to the other bottles. But of course, if the pressure is long continued, bottles of glass of the size named, however well made, would be liable to such an accident; and, if the apparatus is to be remounted, it would be better to use a metallic vessel for the generator, and to cover the glass bottles with some kind of netting. The experience with the glass apparatus led us to seek to adapt to the same purpose some one of the various soda-water apparatuses which are greatly used in the United States for the

production of effervescing drinks. After examining several of the patterns in the market, we selected for trial the one represented below, which is manufactured by the firm of John Matthews, of New York, at their establishment,—First Avenue, 26th and 27th Streets,—in that city. The apparatus was designed by them for preparing that overcharged aqueous solution of carbonic dioxide, which in the United States is familiarly called soda-water; but with a very slight modification it can be used with equal efficiency for the preparation of a similar solu-

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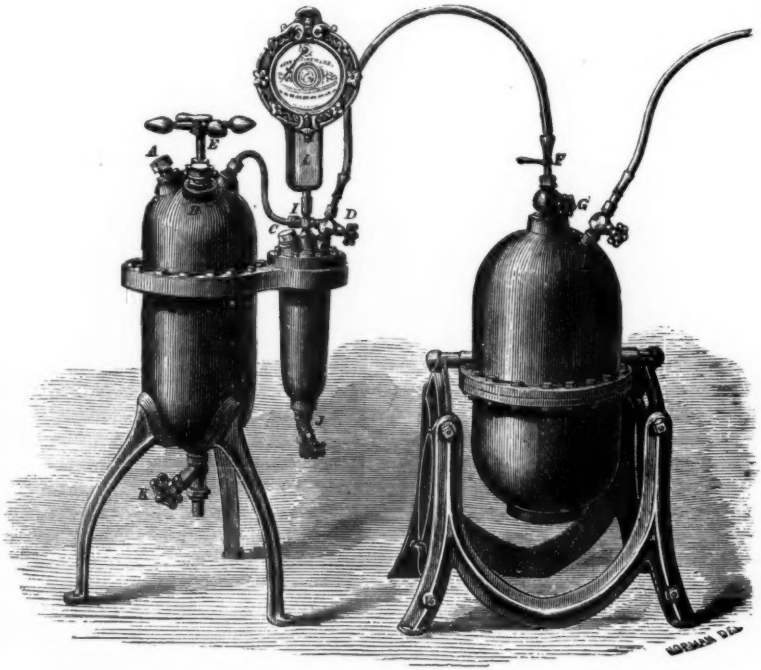

tion of hydric sulphide. We have worked out the details of the process, which this new application involves, and publish them with the hope that the apparatus may be found of as great value in other laboratories as in our own. The wood-cuts are taken from the catalogue of the firm, who have kindly loaned the blocks for this paper; and we would here express our obligations to them for their great courtesy and liberality during the whole course of our experiments.

The apparatus consists of two parts, the generator and the

fountains; and in Fig. 1 the generator is represented connected by a rubber hose with one of the fountains, of which in practice we use three, connected in a line by similar lengths of rubber hose, like so many Woolf's bottles. In the figure, only the first of the line is represented, which is set on trunnions in a frame, in order to facilitate the agitation of the water and the gas. Only one of these frames, however, is required, to which the other fountains can readily be transferred. A section of the generator is represented in Fig. 2. It is made of cast iron,

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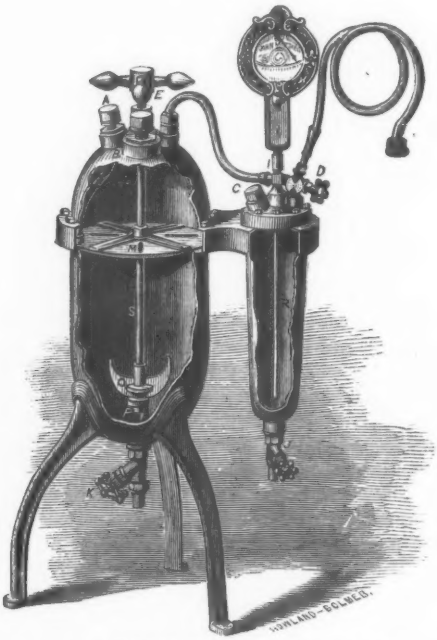
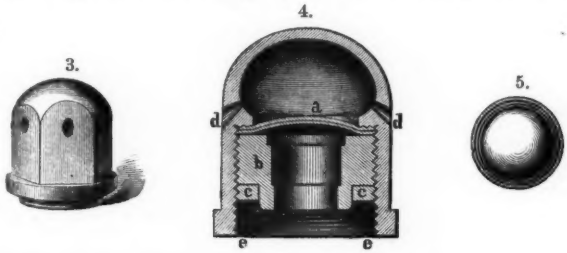


and in two parts (readily distinguished in the figure), which are firmly bolted together, so as to confine in its place the bell-metal plate M, which separates the upper from the lower chamber. In the lower chamber is placed dilute sulphuric acid, which is poured in through the bung A. In the upper chamber, and resting on the plate, is placed coarsely powdered sulphide of iron, which is put in through the bung B. Through the plate M, and on two lines at right angles to each other, there are cut radial slits, half an inch wide, which are guarded by four iron arms. These arms are attached to the agitator shaft S, and move over the surface of the plate, alternately

covering and uncovering the slits, when the handle E is turned. To the lower end of the same shaft is fastened the agitator O, which is turned simultaneously with the arms just mentioned. After the apparatus has been charged, it is evident that by turning the handle the sulphide of iron may be sifted down at pleasure into the acid water below; and the handle and arms are so disposed that when the bungs are uncovered by the handles the slits are covered by the arms. From the generator, the gas passes through the washer R, whose relations to the apparatus, as well as that of the pressure gauge, are evident from the figure. The generator is lined with lead, and the rest of the apparatus, including the bell-metal plate M, with pure tin. The lead lining of the generator is seamless and very heavy, and all the cocks are lined with block tin tubing. Of the various valves, bungs, and stuffing boxes indicated in the figure, it is unnecessary to speak in detail. It is sufficient to say that they are of excellent workmanship, and during a year's trial have kept perfectly tight. The charging bung, B, however, is closed by a safety cap of peculiar construction, which deserves special mention, because it insures the safety of the apparatus. The cap is represented by Fig. 3, and a section is given in Fig. 4.



It will be seen by the last that the escape of the compressed gas from the generator through the apertures *d* is only prevented by a thin disk *a*, which is shown in detail by Fig. 5. This disk is made of two thin plates: the lower one, which comes in contact with the acid spray, is of lead, and the upper one of silvered copper, whose thickness is so adjusted that it must be at once ruptured if the pressure in the apparatus should become unduly great.

Although the so-called soda-water fountains—in which the solution of the gas and water is made—have all in general the same simple relation of parts, they are constructed of very different materials and after very different methods. The outside shell may be either of hammered copper or of some form of iron, and this may be lined with tin, enamel, or glass. After having determined by experiment that a solution of hydric

sulphide—especially when some carbonic dioxide is added—exerts no action on a surface of metallic tin, except a very slight and superficial staining, we selected as best adapted to our purpose the steel fountains, also manufactured by the firm of John Matthews, Fig. 6. These are made of plates of steel

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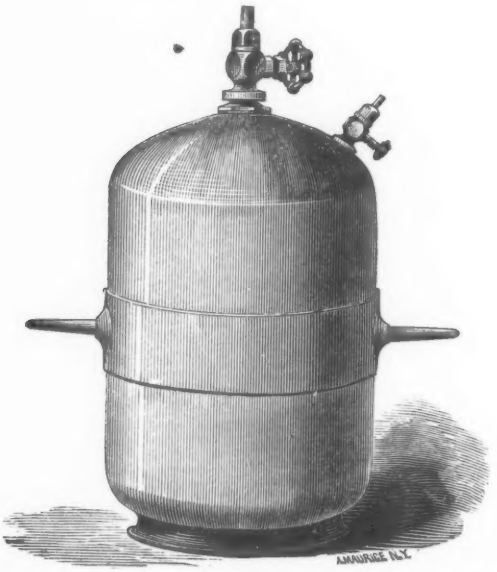

united in a peculiar way invented by themselves so as to secure with comparative lightness very great strength. They are lined on the inside with sheet tin, and the tin lining forms an independent vessel, which alone is connected with the bungs. The tubes and valve cocks are also either made or lined with tin, so that the solution never comes in contact with any other metal. For making ordinary soda-water, the fountain requires only a single valve, which connects with a tube leading to the bottom of the vessel, and this serves both to charge the fountain and to draw off the solution when made. But since a solution of hydric sulphide is rendered turbid if left in contact with even a small quantity of air, and since a variable amount of free hydrogen is always formed by the action of sulphuric acid on common sulphide of iron, it was necessary for our purpose to add to the ordinary fountain a vent cock as shown in the last figure. This enables us to connect together several fountains after the manner of Woolf's bottles, as already described, and, by passing the current of gas through the whole

line, to drive out all the air originally in the apparatus, as well as the free hydrogen subsequently evolved. Moreover, in the preparation of the solution of hydric sulphide, the generator should be much larger relatively to the fountains than in the preparation of soda-water. Thus we use for charging the so-called six-gallon steel fountains, the "Upright Generator," known as No. 40, which has a capacity below the diaphragm of sixty liters. It should be here stated that the fountains are only rated at two-thirds of their total capacity, which is the volume of soda-water which is ordinarily made in them. They should never be more than three-fourths filled with water. But, even with this liberal allowance for gas room, the six-gallon fountains referred to will hold twenty-five liters of water. It may also be stated, although the fact must be evident, that a single large fountain will not give as good results as several small ones of equal capacity; since in expelling the air and free hydrogen, as we have described, there would be a considerable loss of material, if only one fountain were used. Still, in laboratories where the consumption is not great, the loss is not important; and the smallest "Upright Generator," with one six-gallon fountain, will be found to be a perfectly satisfactory apparatus.

Assuming first that the larger apparatus is used, the method of charging is as follows: The large generator is too heavy to be readily moved, and should be placed so that the discharge valve will empty directly into a drain. Water also should be brought to the apparatus by hose,—if possible, both hot and cold water,—not only for convenience in charging, but also in washing. Finally, there ought to be a good flue in the neighborhood, into which the waste gas may be discharged. The apparatus having been thus established, the three fountains—first rinsed out—are filled each with twenty-five liters of distilled water, and the valves having been secured, the fountains are connected with each other and the generator by means of stout rubber hose as already indicated, and the vent valve of the last fountain is connected with the flue by a length of common gas hose. The generator is then charged as follows: The handle is first placed so as to uncover the bungs and cover the slits in the diaphragm. Through the bung A is now poured forty liters of *hot* water, heated to between 70° and 80° C., and then five liters of common oil of vitriol are poured very cautiously into the same bung through a lead funnel with narrow spout provided for the purpose. The acid water will thus be heated nearly to the boiling point, and its capacity of dissolving ferrous sulphate raised to the maximum. Next six kilograms of sulphide of iron—previously sufficiently pulverized to pass through a wire sieve with eight meshes to the inch, and

mixed with two kilograms of marble powder—is run through a wide-mouth funnel into the bung B. The bungs having been now all closed, the joints of the apparatus tested, the exit valve D of the generator and all the valves of the fountains opened, a small amount of the powder is sifted down into the acid water by turning the handle not more than a single revolution, always taking care to leave it so that the slits of the diaphragm shall be left covered. The gas is then allowed to stream through the apparatus, and tested as it escapes into the flue, until it burns without explosion, using a small pneumatic trough for that purpose. The air is now nearly expelled, and the escape of gas should be reduced by the last vent valve until the current only supplies a small jet, which may now be attached and lighted with safety, and by the size of this flame the vent may afterwards be regulated. The handles are now slowly turned, and the pressure in the apparatus carried up to about 120 pounds. The valves of the fountains and the valve D of the generator are now all closed, and the two last fountains detached, leaving the first (which is mounted on a frame as represented in Fig. 1) united to the generator as before. This is now rocked for several minutes to and fro, in order to thoroughly agitate the water with the gas. The stop valve G should then be opened, and then the valve D (very gradually), so that the gas may be admitted slowly to the fountain. The valves are then again closed, and the agitation renewed, and the same operation is repeated several times until no more gas is absorbed by the water in the fountain, the pressure in the generator meanwhile being maintained at 120 pounds, by turning the handle. The first fountain is then removed, and the same process repeated with each of the others. At the close of the operation, after all chemical action has ceased, there remains in the generator—both free and dissolved in the liquid residue—a large volume of hydric sulphide gas. This we economize by venting the generator slowly through Woolf's bottles containing aqua ammonia, and thus preparing at the same time ammonic sulphide. It is not unimportant to add that the generator should be emptied before it cools, and the ferrous sulphate has time to crystallize. The discharge valve R should then be removed, and the whole apparatus thoroughly washed out. If the valve becomes clogged, it can generally be cleared by developing pressure in the generator by means of a small charge of ground marble. Otherwise, after the pressure has been relieved, and the upper valves and bungs closed, the discharge valve must be cautiously removed, and an opening forced through the lower bung. Such details are necessary in order that others may profit by our experience; but, lest the details should convey the impression that

the apparatus is complicated, and that the process requires skilled labor, it may be stated that in this laboratory the apparatus is entirely in charge of a laboratory servant, and may be managed by any workman of ordinary intelligence. It does not require more than four hours to work over a single charge, and this yields seventy-five liters of hydric sulphide solution under a pressure of 120 pounds. According to our experience, this supply lasts with a class of one hundred students in qualitative analysis about a month. For laboratories where the classes are much smaller than this, we should recommend the smallest size generator, which may be used with a single six-gallon fountain. The smaller apparatus is managed in precisely the same way as the larger, only taking one half the quantity of materials. It is important as before to wash out in the first place all the air, and to maintain a small current of gas through the vent valve of the fountain while the pressure in the apparatus is rising. This of course entails some loss of hydric sulphide; but the chief loss is caused by the venting of the generator, and all this gas may be utilized for the preparation of alkaline sulphides as described above, or this waste gas may be used for washing out the air from a second fountain, and thus preparing it for a subsequent charge.

For dispensing the reagent in our *qualitative* laboratory, we place the fountain in a cupboard and connect it by a block-tin tube with a so-called "draught column," Fig. 7, in the ventilating hood above. From this each student draws the reagent as it is required. In quantitative work we may draw the charged water directly from the fountain into the midst of the solution to be treated, using simply a common rubber hose terminated by a glass tube which dips under the surface of the liquid: the hose is attached to the nipple of a cap which screws on to the valve of the fountain, and by confining two or more disks of linen cloth between the cap and the head of the valve (like washers) we can filter the solution as it flows out. It is more convenient, however, to distribute the reagent to advanced students in the well-known soda-water siphon, Fig. 8. These require no description, but as the simple apparatus used for filling the siphon is not yet a familiar object in chemical laboratories, we give a figure of the apparatus (Fig. 9), taken, like our other cuts, from the catalogue of John Matthews. By

7.

pressing the foot on a pedal shown at the base of the apparatus, the siphon—confined in a cage—is raised, so that its mouth is forced tightly against one opening of a valve of peculiar construction, the second opening of which is united by a block-tin

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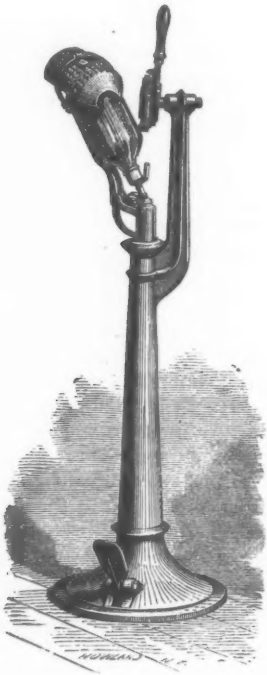


tube to a fountain; while at the same time the handle of the siphon is pressed back. On now pushing the upper lever shown in the cut to the right, the valve of the fountain having been previously opened, a connection is made between the fountain and the siphon; when the charged water rushes into the latter, condensing the air to a fraction of its original volume. If next the same lever is pushed momentarily to the left, the first connection is closed; while a vent is opened, through which the condensed air rushes out into the atmosphere before the gas in solution has time to escape, and then on pushing back the handle a further portion of liquid enters, nearly filling the interior of the bottle. Lastly, on raising the foot, the valve of the siphon shuts at the same time that the

bottle is lowered and released from its cage,—the whole process being the work of a few seconds. At the first filling, there sometimes remains a small amount of air in the siphon, so that the solution becomes slightly turbid on standing; but this effect of oxidation can be easily avoided by using a sufficient amount of marble powder in charging the generator. This siphon unquestionably affords the most convenient mode of using hydric sulphide; and for a private laboratory a single siphon, well charged, will afford as ample a supply as will ordinarily be needed. It is greatly to be hoped that our manufacturing chemists may soon find an advantage in supplying this important reagent in such an elegant form.*

When the water is charged as directed above, it of course holds in solution, besides hydric sulphide, a considerable volume of carbonic dioxide; and if, under any circumstances, the presence of this last gas would produce an injurious effect, the marble powder can be simply omitted in charging the generator. In almost all cases, however, the carbonic dioxide exerts a very beneficial influence, and in several ways. In the first place, it insures the non-action of the hydric sulphide on the metallic surfaces of the apparatus. In the second place, it protects the solution from the action of the air when it is drawn into an open vessel, so that after a metallic sulphide has been precipitated by an excess of the reagent, the products may be digested in an open flask or beaker without fear of oxidation. In the third place, the carbonic dioxide adds greatly to the tension of the confined gas, and enables us to develop sufficient pressure to charge the siphon without unnecessarily increasing the strength of the solution of hydric sulphide.

Chemical Laboratory of Harvard College, 1876.

ART. XLVIII.—*Report on the Physical Investigations carried on by P. Herbert Carpenter, B.A., in H.M.S. "Valorous" during her Return Voyage from Disco Island in August*, 1875; by WILLIAM B. CARPENTER, C.B., M.D., F.R.S.†

IN the first of the Serial Soundings taken by the "Valorous," nearly in the middle of Davis Strait and on the parallel of Godthaab, the bottom-temperature, at a depth of 410 fathoms, was 34·6° Fahr.; and the descent to this from a surface-temperature of 40° was nearly uniform—39°, 38°, 37°, 36° and

* All the apparatus here described may be obtained from the firm of John Matthews, First Avenue, 26th and 27th Streets, New York, at very reasonable rates. Be careful in ordering to state the use to which the apparatus is to be put, with the caution that no silver plating or lead paint should be used upon it.

† Proc. Roy. Soc., vol. XXV, No. 173, p. 230, June 15, 1876.

35° being met with at almost equal intervals. There was here, therefore, no indication of any contrary movement of different strata of water, or of any special superheating of the superficial stratum. But the case was very different with the next much deeper sounding which was taken about a degree further south, but still toward the middle of Davis Strait: for there was here a surface-stratum of 45°, but of such extremely small thickness, that the isotherm of 40° was reached in about fifteen fathoms; from 40° to 38° the interval was nearly the same as in the previous sounding; but below 38° the descent was so slow that 37° was not reached until nearly 800 fathoms, and on the bottom at 1100 fathoms the temperature was still 36·4°. At the next station, latitude 62° 6′ N., longitude 55° 56′ W., (that is, another degree further south, and at about the same distance from the Greenland coast), a depth of 1350 fathoms was met with; the surface-temperature was still 45°; but the bottom-temperature was found to be 34·6°, as in the 410 fathoms sounding. The next temperature-sounding was taken nearly three degrees further south and five and one-half degrees to the west, namely, in latitude 59° 10′ N., longitude 50° 25′ W.; that is, a little to the south of Cape Farewell, but still six degrees to the west of it: here the surface-temperature was still 45°; but the bottom-temperature at 1,750 fathoms had sunk to 33·4°. Finally, a set of serial soundings was taken before rounding Cape Farewell, about a degree further south and four degrees east: the surface-temperature had then risen to 49°; but the isotherm of 40° was reached at about fifty fathoms, that of 39° at about ninety fathoms, and that of 38° at about 160 fathoms; while below this the descent of the thermometers was extremely slow down to the isotherm of 37°, which lay at about 1050 fathoms—becoming more rapid, however, beneath this, so that 36° was reached at about 1400 fathoms, 35° at about 1500, and 34·3° on the bottom at 1660 fathoms.

Now these phenomena seem to me to point very distinctly to the existence (1) of a superheated layer, which is slowly moving up Davis Strait, and gradually losing its excess of temperature as it proceeds north, as shown by the gradual approach of the isotherms to the surface; (2) of a neutral intermediate layer, 1,000 fathoms or more in thickness, marked out by the extreme uniformity of its temperature, which indicates its stationary condition; and (3) of a deep cold layer, which as clearly derives its low temperature from a northern source, as the uppermost stratum does from a southern, and which must, therefore, be in movement.

The temperatures at Station VI seem at first sight rather anomalous when compared with those of Stations VII–X—the

isotherm of 37° here coming up within 200 fathoms of the surface, while at only a degree farther south it lies at nearly 800 fathoms; and a bottom-temperature of 34·6° being found at 410 fathoms at Station VI, while at Station VIII it is only reached at 1,350 fathoms. But the anomaly disappears when the rapidly increasing depth and the tendency of the coldest water to gravitate to the bottom are taken into account: for it appears, from the temperature-soundings taken further north toward Disco Island by the Swedish ship "Ingegera," that water as cold as this, and even much colder (31° being recorded in one instance), is there found at depths varying between fifty-eight and 185 fathoms; and it can scarcely be doubted that the water which is chilled by the more severe cold of Baffin's Bay is here flowing down the slope of Davis Strait. Again, it is at first sight an anomaly to find at Station VIII a bottom-temperature of 34·6° at 1,350 fathoms, while the bottom-temperatures both to the north and to the south of it are 34·6°; but this only shows that the coldest polar water is flowing south through some deeper channel, perhaps in the western half of Davis Strait.* And the same explanation applies to the yet more remarkable fact that a bottom-temperature of 33·4° was met with near the mouth of Davis Strait, when no such water was met with further north. But that even this does not carry down the coldest water of the Arctic basin, is obvious from the fact brought to light by the "Porcupine" temperature-soundings in the "Lightning Channel," (between the north of Scotland and the Faröe Islands), over a large part of whose bottom we found the temperature to range two degrees, or even more, *below* 32°.

The next temperature-sounding, taken on the 17th of August almost exactly in the meridian of Cape Farewell, and not quite two degrees to the south of it, gave, like No. IX, a bottom-temperature of 33·4° at 1,860 fathoms; so that it seemed pretty clear that this is the temperature of the coldest water that can find its way into the North Atlantic along either the west or the east coast of Greenland. And from the depth at which the isotherm 35° was found to lie in the 1660 fathoms serial sounding, it is obvious that the stratum between 35° and 33·4° must be here a very thin one; while the upward slope

* As I pointed out on a former occasion, (Proc. Roy. Soc., vol. xx, p. 624, § 144), any water moving from either pole toward the equator will have a *westerly* tendency in virtue of its *deficiency* of easterly momentum; just as water moving from the equator toward either pole will have an *easterly* set, in virtue of the *excess* of easterly momentum which it carries with it. The later temperature-soundings of the "Challenger" in the South Atlantic have given the explanation of the temperature of 32·4° observed under the equator in the first year of her voyage, but not encountered in any of the earlier temperature-soundings taken in the South Atlantic, by showing that the coldest Antarctic underflow is met with on the *westerly* part of its sea-bed.

which is indicated by the next sounding, shows that it must rapidly die out toward the east.

The course of the "Valorous" having then been kept at first nearly due east, and afterward southeast, another serial temperature-sounding was taken on the 19th of August, in latitude 56° 11′ N., and longitude 37° 41′ W. The surface-temperature had here risen to 53°,—about the same as we had encountered in the "Lightning Channel," at the same time of the year, rather farther to the north; but the warm upper stratum was here thinner, a reduction to 45° taking place within fifty fathoms, and to 40° within 300; whereas in latitude 59° 35′ N., longitude 9° 11′ W., we had found the isotherm of 45° lying below 500 fathoms, while the bottom at 767 fathoms was still 41·4°. It is obvious moreover, from the regularity of the descent of the isotherm of 40° in this part of the North Atlantic, that *easting* has more influence on the rate of that descent than *southing*—thus confirming the view formerly expressed as to the tendency of the warm upper flow toward the *eastern* side of the basin.* The isotherms of 39° and 38° slope downward toward the east at about the same rate; but those of 37° and 36° still nearly keep their parallelism to the surface, confirming the previous suggestion of the "neutrality" of the deep stratum which they underlie.

Between the last station and the next, taken in latitude 56° 1′ N., and longitude 34° 42′ W., in the line of the channel between Iceland and Greenland, but considerably to the south of it, the sea-bed was found to have shallowed most remarkably, bottom being struck at 690 fathoms, and the bottom-temperature rising again to 38·2°. This elevation may be regarded with great probability as a continuation of that which was encountered by Sir L. McClintock in the line of temperature-soundings which he took several years ago across the North Atlantic between Rockall and Cape Farewell; for almost exactly in a line between the "Valorous" Station 13 and Iceland, Sir L. McClintock met with bottom at 743 fathoms, between 1,260 fathoms on the east and 1,159 fathoms on the west.

The course being now again kept nearly due east, another temperature-sounding was obtained in latitude 55° 58′ N., longitude 31° 41′ W., which, on a bottom of 1,230 fathoms, gave a bottom-temperature of 36·8°, the surface-temperature being 54·5°. Three degrees farther east, and on the same parallel, another set of serial temperatures was taken, which indicated a further increase in the upper warm stratum, the isotherm of 40° descending to about 380 fathoms; but the

* Shearwater Scientific Researches, 1872, §§ 144, 148, (Proc. Roy. Soc., xx, pp. 624, 626).

depths of the isotherms of 39°, 38° and 37° show little change; and the bottom at 1,485 fathoms was 36·5°, as at the corresponding depth on the other side of the ridge. Still farther to the east, in latitude 55° 10′ N., longitude 25° 58′ W., the depth was found to have still further increased to 1,785 fathoms; but the bottom showed no lower a temperature than 36·7°, although in the 1,750 fathoms sounding on the other side of the ridge the thermometer fell to more than *three degrees lower*.

Bad weather having come on, it was not considered prudent, in the disabled condition of the ship, to attempt further scientific explorations; and the course was accordingly shaped for Cork.

The Temperature-Section prepared from the serial soundings taken in the "Valorous" after quitting Davis Strait has been continued toward Valentia on the basis of the serial soundings taken off the coast of Ireland in the first cruise of the "Porcupine" in 1867, a sounding in 1,263 fathoms, latitude 56° 8′ N., longitude 13° 34′ W., being taken as the principal guide. This being almost on the same parallel with the last serial sounding of the "Valorous," (the difference of latitude being only half a degree), and the seasonal difference being rather in favor of the "Valorous" temperatures, it is extremely striking to find in this section the most remarkable contrast yet brought out between the thermal condition of the eastern and the western sides of the North Atlantic: for the descent of all the isotherms as they pass from west to east, which has been already pointed out in the "Valorous" portion of the section, continues at an even more rapid rate; so that the isotherm of 40°, which lay at Station XVI at 380 fathoms, lies at 900 fathoms at Station XVII, fifteen degrees to the west; while the isotherm of 45°, which at the first of these stations lay at eighty fathoms from the surface, lay in the second at 640 fathoms. This difference in the thickness of the whole stratum above the isotherm of 40° is much more remarkable than the difference of surface-temperature, the increase of which between the first and the second station was only from 55° to 59·6°.

It is clear, therefore, that the heating power of the warm flow which comes up from the southwest toward the western shores of the British Isles, and which proceeds onward to the northeast, so as to ameliorate the climate of the Orkneys and Shetland Islands, but still more markedly to affect that of the coast of Norway (as has been shown by Professor Mohn), depends upon *its great depth*. Any such superheated film as the Gulf-stream has been found to be when last recognizable as a current (as was long since urged by Mr. Findlay, and has since been confirmed by Capt. Chimmo's observations), must lose its excess of warmth long before it reaches our shores.

Hence, as I have urged on a former occasion, the prolonged heating power of the northeast flow depends much more upon the thickness of its moderately warm stratum than upon its bringing with it a high surface-temperature. A layer of fifty fathoms at 60°, flowing northeast over a bed of ocean-water at 40°, and exposed above to an atmosphere of 40°, would be cooled down to that standard in two or three weeks. But a layer of 900 fathoms thickness, ranging from 40° to 55°, would retain an excess of temperature far longer.

The advocates of the doctrine that the *vis a tergo* is the Gulf-stream, which cannot be traced as a current by any distinctive feature further to the northeast than the parallel of 40° and the meridian of 30°, have to show in what way it can raise the temperature of so thick a stratum of ocean-water as we have seen to be affected in the western portion of the North Atlantic by a warm flow of some kind. Whether, as Professor Wyville Thomson maintains, the approximation of its boundaries between the British Islands on one side and Labrador and Greenland on the other can possibly produce this result, is a point on which it is for hydrographers to decide. For myself, I cannot regard it as probable that a spent stream of fifty fathoms thickness can give motion to a vast layer of 900 fathoms depth.

On the other hand, the doctrine I advocate, that a thick upper stratum of the North Atlantic is slowly moving poleward, to fill up the void left by the gravitation-underflow of the coldest water toward the equator, and that this stratum will also have an easterly tendency in virtue of the excess of easterly momentum which it brings with it from a lower latitude, seems adequately to account for the facts now brought to light. The progressive closing in of the boundaries of this poleward upper flow will obviously tend to deepen it, so as to give it a more persistent heating power.* In the South Atlantic and Southern Indian Oceans, on the other hand, the progressive opening-out of the ocean-boundaries, as we pass southward from the equator, will tend in the same measure to reduce the thickness of the poleward upper flow, thus diminishing the persistence of its heating power. And in this, as it seems to me, we have the true explanation of the marked difference between the climate of Kerguelen's Land (latitude 50° S.), for

* This position may seem inconsistent with the objection just taken to the doctrine of Sir Wyville Thomson. But the inconsistency is only apparent. I cannot conceive that after the Florida Current has spread itself out like a fan over the Mid-Atlantic, it can retain enough *vis a tergo* to give a northeast movement to a mass of water nearly 2,000 miles wide and 700 or 800 fathoms deep, the impelling force being progressively weakened by the obstacles to that movement. On the other hand, the force which (on the doctrine of a Thermal circulation) acts as a *vis a fronte*, grows stronger as the water which it puts in motion approaches the polar area, and thus is fully competent to deepen the poleward stratum in proportion to the reduction of its breadth.

example, or that of Heard Island (latitude 53° S.), and that of Ireland, (lying between the parallels of 51½° and 54½° N. latitude), the summer temperature of the former being but little above the winter temperature of the latter.

The "Challenger" temperature-sections have most conclusively shown that the entire warm upper stratum in the South Atlantic is very much thinner than that of the North Atlantic; and while I fully admit that a part of this difference is due to the fact that a far larger portion of the equatorial current is deflected into the latter than into the former, I cannot see that the Gulf-stream by any means accounts for the descent of the isotherm of 40° in latitude 56° N. to a depth of 900 fathoms.

The "Valorous" temperature-soundings seem to me to be of peculiar interest and value, in furnishing a satisfactory explanation of the comparatively high bottom-temperature of the North Atlantic. I have always attributed this to the comparative narrowness of the channels of communication between the Arctic and the North Atlantic basins, which restrict the flow of the coldest polar water from the former into the latter; and long before the "Challenger" Expedition sailed, I had ventured the prediction that the South Atlantic, on account of the perfect freedom of its communication with the Antarctic, would have a colder bottom, and that the influence of the Antarctic underflow would probably extend to the north of the equator. By Sir Wyville Thomson, on the other hand, it was argued from the commencement that the whole cooling of the deep stratum of the *North* Atlantic is due to the *Antarctic* underflow; and this conviction he repeats in his last utterance on the subject, on the ground of the continuity of the isotherms from the South into the North Atlantic.* The question arises, however, why the deep stratum of the North Pacific, which is undoubtedly fed from the Antarctic, should be so decidedly colder, as the "Challenger" and "Tuscarora" soundings show it to be, than the deep stratum of the North Atlantic; and this question appears to me to find an entirely satisfactory answer in the indication furnished by the Second Section, that the Arctic Basin is for the most part separated from that of the North Atlantic by an intervening ridge, which (like many similar ridges discovered by the "Challenger") allows water of about 36°, but *not colder water*, to pass from the former into the latter. The limited contributions of colder water furnished by Baffin's Bay and the "Lightning Channel" would help to reduce the deep temperature of the North Atlantic generally to the 35°–36° shown in the "Challenger" Sections; but it is only when, on approaching the equator, a bottom-temperature below this first shows itself, that I can recognize the influence of the Antarctic underflow.

* Proceedings of the Royal Society, vol. xxiv, p. 632.

I forbear, however, to discuss this subject more fully at present, the Admiralty not having yet published the final installment of the "Challenger" temperature-sections. And I shall confine myself to an expression of my earnest hope that the ship to be sent next year to communicate with the Arctic Expedition may have, as part of its work, the completion of that which the "Valorous" was disabled from performing—namely, the obtaining a continuous temperature-section between Iceland and Greenland, and another across Davis Strait.

ART. XLIX.—*On the Influence of Geological Changes on the Earth's Axis of Rotation*;* by GEORGE H. DARWIN, M.A., Fellow of Trinity College, Cambridge. Received by the Royal Society October 13, 1876.†

THE subject of the fixity or mobility of the earth's axis of rotation in that body, and the possibility of variations in the obliquity of the ecliptic, has of late been attracting much attention; but the author believes that it has not hitherto been treated at much length. The paper, of which the following is an abstract, is an attempt to investigate the results of the supposition that the earth is slowly changing its shape, with especial reference to the effects on the obliquity of the ecliptic and on the geographical position of the earth's axis of figure.

1. This part of the paper is devoted to the consideration of the precession and nutations of an ellipsoid of revolution which is slowly and uniformly changing its shape. The change is supposed to proceed from causes internal to the earth, and only to continue so long as the total changes in the principal moments of inertia C and A remain small compared to their difference, C—A.

The problem is treated by means of M. Liouville's extension of Euler's equations of motion of a rigid body about a point.† By an approximate method these equations may be treated as linear, and the solution divided into two parts.

Let θ be the obliquity of the ecliptic; Π cosec θ the precession of the equinoxes; $-n$ the angular velocity of rotation of

* Since this paper was in manuscript Sir William Thomson has delivered his address to the Mathematical Section of the British Association at Glasgow. He therein touches on this subject, and gives some of the results attained here; but as he has not stated how he has attacked the problem, and as the subject has been recently attracting much attention, the author still ventures to offer his paper to the Royal Society.

† Abstract of Mr. Darwin's Memoir, from the Proceedings of the Royal Society xxv, No. 175, 328.

‡ Liouv. Journ., 2e série, t. iii, 1858, p. 1; Routh's Rigid Dynam, p. 150.

the ellipsoid; $A+at$, $A+bt$, $C+ct$ the principal moments of inertia at the time t. Then it is shown that the secular effect on the obliquity of the ecliptic, as resulting from the motion of the principal axes in the body (which constitutes the first part of the solution), is given by the equation

$$\frac{d\theta}{dt}=-\frac{\Pi}{2n}\frac{a+b-2c}{A};$$

and as resulting from the change in the impressed forces, due to the change of shape of the body (which constitutes the second part), is given by

$$\frac{d\theta}{dt}=\frac{\Pi}{2n}\frac{a+b-2c}{C-A}.$$

The former part may be neglected compared with the latter. But from such geological changes as we are entitled to assume in the case of the earth, the total change in the obliquity of the ecliptic must be exceedingly small. Even gigantic polar ice-caps during the Glacial period could not have altered the position of the arctic circle by so much as three inches; and this is the most favorable redistribution of matter on the earth's surface for producing that effect. Thus the obliquity of the ecliptic has remained sensibly constant throughout geological history.

It is also shown that, during any gradual deformation of the ellipsoid, the instantaneous axis of rotation will always remain sensibly coincident with the principal axis of figure.

In the course of the work by which the previous results are attained there is shown to be a small inequality in the motion of the instantaneous axis, in consequence of which that axis describes a circle with uniform velocity, and is coincident with the axis of figure every 306th day (in the earth). This circle touches the meridian along which the axis of figure is traveling in consequence of the deformation of the earth's shape. The diameter of the circle is shown in a particular case (not unfavorable to produce a large effect) to be less than $\frac{1}{5}\frac{1}{7}\frac{1}{5}''$. But although this inequality appears to be so small, it is of interest and is discussed at some length. It is shown that, if the earth be not quite rigid, this inequality might have the effect of modifying the path of the axis of figure in the body, in consequence of readjustments to a figure of equilibrium.

Various hypotheses as to the power of adjustment are considered, and the paths of the instantaneous and principal axes in the precession of a viscous spheroid undergoing deformation are found.

It is maintained that although the earth may be sensibly rigid to the tidally deforming forces exercised by the sun and moon,

it would not be so to considerable departures from the figure of equilibrium, such as would arise from a wandering of the pole of figure from its initial position; and that readjustments to an approximate form of equilibrium probably take place, at considerable intervals of time, impulsively by means of earthquakes. Such periodical adjustments would not sensibly modify the geographical path of the principal axis as due to terrestrial deformation. But it is held that during the consolidation of the earth there must have been great instability in the geographical position of the poles. Throughout the rest of the inquiry, however, the hypothesis of the earth's sensible rigidity, together with the possibility of more or less rare impulsive readjustments to the figure of equilibrium, is adhered to. In consequence of these results dynamical considerations may be dismissed, and it only remains to consider the kinematical question of the change in the earth's principal axes due to any deformation of its shape.

2. Formulæ for this end are here found, and are adapted for numerical calculation. It is assumed, in the first place, that the deformation is such that there is no change in the strata of equal density; and accordingly all suppositions as to the nature of the internal changes accompanying geological upheaval and subsidence are set aside.

3. The forms of continent and depression are next investigated, which, for the transport of a given quantity of matter from one part of the earth's surface to another, would cause the maximum deflection of the principal axis of greatest moment—subject, however, to the condition that the layer excavated or piled up shall nowhere exceed a given small fraction of the earth's radius.

It is shown that the continents and depressions must be of uniform height and depth; there must be two of each, all similar to one another; that each has one of its own kind diametrically opposite to it; that they are in shape sphero-conics, formed by the intersection of a certain elliptic cone with the sphere; that the centers of the four sphero-conics are all on the same *complete* meridian and all in latitude 45°. A table of numerical results depending on the values of certain elliptic functions is given.

4. In this part an endeavor is made to collect evidence as to the extent to which the earth may have undergone deformation from geological changes. The object is to discover what are the largest areas over which there has been a consentaneous rise or fall, and what is the greatest vertical amount of that rise or fall; also to determine how the erosion of the land and the sea affect the local excesses or deficiencies of matter on the earth's surface. The areas and amounts of elevation and subsidence

which on a sealess and rainless globe are equivalent, as far as producing excesses or deficiencies of surface matter, to those which obtain on the earth are referred to as "effective;" and it is only the effective elevation or subsidence which we require to know in order to determine the shift of the earth's axis.

The evidence as to area is very meager, because precise boundaries to regions of elevation and subsidence cannot be assigned; but, *faute de mieux*, the author's father, Mr. Charles Darwin, marked out for him on a map an area in the Pacific Ocean which (on account of the structure of the coral islands) he believes to have undergone subsidence within a recent geological period. From a consideration of this and of other points the author believes that from $\frac{1}{1\,0}$ to $\frac{1}{2\,0}$ of the whole earth's surface may, from time to time, have undergone elevation and subsidence. The greatest vertical effective amount of rise or fall cannot be determined from geological evidence, because of the effects of erosion and of the influx of the sea into parts below the mean level of the earth.

The only way of determining the point seems to be to find what is the difference of mass, standing on unit area of the earth's surface, in an ocean of, say, 15,000 feet deep, and in land of, say, 1100 feet high. From this difference of mass the effective elevation of an ocean-bed in its conversion into land can be at once determined. Taking the above numbers, it is found to be 10,436 feet; and in the examples given in the following part, the deflection of the polar axis, for an assumed effective elevation of 10,000 feet, is given in each case.

It is then pointed out that, if the deformation of the earth were of very wide extent, the level surface of the sea would approximately follow the rocky surface, and that thus there might be sufficient change in the earth's shape to sensibly affect the position of the principal axis, without there being any geological signs of elevation or subsidence.

5. Numerical application is now made of the preceding work to the case of the earth, and, as before stated, all the results are given for 10,000 feet of effective elevation.

The first application is to continents and seas of maximum effect, and a table of results is given. It may be here stated that if $\frac{1}{3\,0}$ of the earth's surface is elevated, the deflection of the pole is $11\frac{1}{3}'$; if $\frac{1}{2\,0}$, $1°\ 46\frac{1}{2}'$; if $\frac{1}{1\,0}$, $3°\ 17'$; and if $\frac{1}{2}$, $8°\ 4\frac{1}{2}'$.* In each case an equal area is supposed to fall simultaneously.

Other examples are then given for continents and seas which do not satisfy the maximum condition; in some the boundaries are abrupt cliffs, in others shelving.

The conclusion is arrived at, that a single large geological

* The area of Africa is about ·059, and of South America about ·033 of the earth's surface.

produce an alteration in the position of the pole of from one to change, such as those which obtain on the earth, is competent to three degrees of latitude, on the hypothesis that there is no change in the law of internal density.

6. Various hypotheses as to the nature of the internal changes accompanying the deformation of the earth are discussed.

First, it is shown that if upheaval and subsidence are due to a shrinking of the earth as a whole, but to the shrinking being quicker than the mean in some regions and slower in others, the results are the same as those previously attained.

Second, the increase of surface matter due to the deposit of marine strata also gives the same results.

Third, the hypothesis that upheaval and subsidence are due to the intumescence or contraction immediately under the regions in question is considered. Under certain special assumptions, too long to recapitulate, it is shown that the previous results must be largely reduced. A table of the values of the reducing factor for various thicknesses of the intumescent strata is given; from which it appears that if the stratum is tolerably thin and at all near the surface, the deflection of the pole is reduced to quite an insignificant amount. Even if the intumescence extends right down to the center of the earth in a cone bounded by the elevated region, the results would be only about $\frac{3}{4}$ of the former ones. Hence it appears that the earlier results can only be stated as a superior limit to what is possible.

7. In conclusion it is pointed out that if the earth be quite rigid, no redistribution of matter in new continents could ever cause the deviation of the pole from its primitive position to exceed the limit of about 3°. But if the previously maintained view is correct, that the earth readjusts itself periodically to a new form of equilibrium, then there is possibility of a cumulative effect; and the pole may have wandered some 10° or 15° from its primitive position, or have made a smaller excursion and returned to near its old place. No such cumulation is possible, however, with respect to the obliquity of the ecliptic.

It is suggested that possibly the glacial period may not have been really one of great cold, but that Europe and North America may have been then in a much higher latitude, and that on the pole retreating they were brought back again to the warmth. There seem to be, however, certain geological objections to this view.

ART. L.—*On a Base derived from a Waste-product in the Aniline Manufacture;* by C. LORING JACKSON.

IN the spring of 1875 under the direction of Prof. A. W. Hofmann I investigated a waste-product from the factory of Drs. Martius and Mendelssohn-Bartholdy at Rummelsburg near Berlin, which was obtained after the toluidine had passed over in the annual rectification of the highest fractions from the aniline-oil distillation. The results I then obtained were published in the Berichte der Deutschen Chemischen Gesellschaft for 1875, page 968, and may be briefly restated as follows by way of introduction to my subsequent work.

The substance, a black oily liquid, was dissolved in strong hydrochloric acid, and freed from tar by repeated filtration; the oily base set free by sodic hydrate was then fractioned, and yielded at first xylidine and allied bodies which were not further examined; later a fraction from 280° to 320° which, after removal of naphtylamine with dilute sulphuric acid, was converted into a nitrate and purified by crystallization; it then formed radiated groups of white needles which gave on analysis:

	Calculated for $C_{13}H_{14}NNO_3$.	Observed.
Carbon	63·41	63·38
Hydrogen	5·69	5·66

By heating with nitric acid a magenta-colored substance was formed which rendered it very difficult to obtain the salt white.

The *sulphate* was very soluble.

The *chloride* $C_{13}H_{14}NCl$ crystallized in white flat needles.

The *platinum salt* $(C_{13}H_{14}N)_2PtCl_6$ crystallized in very characteristic fan-like groups of sparingly soluble light yellow needles often more than half a centimeter long. The analysis gave

	Calculated.	Found.
Platinum	25·36	25·39
Carbon	40·03	38·78
Hydrogen	3·59	3·82

The *free base* $C_{13}H_{13}N$ was obtained as an oil which gave with chloroform and potassic hydrate a smell similar to that of the isocyanides; it was therefore a primary amine, and its formula must be written $C_{13}H_{11}NH_2$.

An *acet-compound* $C_{13}H_{11}NHC_2H_3O$ was also prepared by the action of acetyl-chloride on the free base; it crystallized in white needles and melted at 114°·25. At that time also some attempts were made to obtain synthetically a base with the same formula, but the results were not published.

At the beginning of the next year, T. Carnelly* described a base $C_{13}H_{11}NH_2$ formed by the reduction of mononitrotolylphenyl made synthetically, but as he did not describe either the acet-compound or the platinum salt there were no data for a comparison of his base with mine; to furnish such data is the object of the present paper. Carnelly found that, on addition of sodic hydrate to a salt of his base and extraction with ether, on evaporating the ether "an oily body was first produced from which there separated out a small quantity of needle-shaped tufts. The melting-point of these after careful drying was found to be 93°–97°." Under the same conditions my base also furnished an oil, but at the time of the investigation in Berlin I observed no crystals. In the hope that longer standing would cause it to solidify I have since the publication of Carnelly's paper, precipitated the small quantity of the salts of my base still at my disposal with sodic hydrate and allowed the brown oil thus obtained to stand under a bell-jar; after several days crystals appeared, and in a few weeks it had changed nearly completely into good-sized prismatic crystals arranged in pennate groups which broke up on pressure into rough octahedral masses. Freed by means of filter-paper from a little adhering brown oil these crystals melted at 46°–70°; from an ethereal solution the substance separated as a brown oil solidifying on stirring to pennate plates with the same melting-point as before recrystallization. The crystals were insoluble in water, freely soluble in alcohol, ether and benzol, and instantly converted by hydrochloric acid into a chloride which dissolved easily in water and was obtained crystallized in white flattened needles two and a half centimeters long melting with partial decomposition near 200°, whereas Carnelly's crystalline base when heated with hydrochloric acid "dissolved only after long boiling; on cooling, small fine white silky needles were deposited, which were separated from the mother-liquor, washed with a little water and dried over lime, after which they were found to melt, with blackening, at 280° to 283°."

From this comparison it is evident that the base described by Carnelly is not identical but isomeric with that obtained by me, and this view is further confirmed by the fact that the fan-shaped crystal-groups of the platinum salt of my base were so characteristic that Carnelly would have certainly described them had his platinum salt appeared in this form.

Finally, it is worth mentioning that in 1875 I had, as announced at the end of my first paper, attempted to prepare synthetically a base with this formula, and even after the publication of Carnelly's paper, as my method was different from his, I determined to bring the experiments for which I had pro-

* Journ. Chem. Soc., II, xiv, 21.

vided material to a conclusion before abandoning the field to him; the work however was delayed by other and more important researches and it was not until this winter that I was ready to examine the products of the reaction of sodium on a mixture of parabromanilin and parabromtoluol; just as I had proved the absence of any base other than bromanilin I received the paper* Ueber eine neue Bildung des Azobenzols by R. Anschuetz and G. Schultz and guided by it have succeeded in detecting also the azobenzol. I can therefore confirm the results recorded in that paper in every particular.

To avoid interfering with any of the above-named gentlemen I intend to abandon this line of research.

Chemical Laboratory of Harvard University, Cambridge, Mass., March 3d, 1877.

ART. LI.—*On an association of Gold with Scheelite in Idaho;* by B. SILLIMAN.

A CORRESPONDENT in Idaho has lately sent me a specimen, obtained from a mineral vein, in which native gold is associated with scheelite and quartz. On account of some resemblance in color to grossular garnet, the miners mistook the scheelite for this species. But the high density of the mass at once attracts attention and a trial for hardness alone proves it is not garnet. The pyrognostic characters also show the absence of silica and the presence of tungstic acid. The powdered mineral treated with hydrochloric acid develops at once the characteristic yellow of tungstic acid, and zinc converts this solution to the familiar blue of tungstic oxide.

The gold appears in the scheelite, intimately associated with a small amount of pyrite, interspersed in the mass of scheelite and quartz. The pyrite is considerably decomposed, yielding yellowish-brown oxide of iron, and some green stains indicate also chalcopyrite, traces of which are detected by close observation. Leucopyrite is also present in minute quantities. But the total quantity of sulphides is certainly not over 1 to 1½ per cent of the mass, if so much.

The scheelite is massive, with few crystalline faces; its color is yellow-brown, resembling chondrodite. Portions of it are quite free from quartz or any mineral but gold. I found the density of the entire mass (weighing 379·42 grams) to be 3·27. Taking the density of quartz =2·66, and of scheelite =6, and disregarding the minor constituents of the mass, the proportions of these two minerals is as 270 : 109·37, or 2·46 parts of quartz to 1 part of scheelite. This is doubtless much above the aver-

* Ber. Deutsch. Chem. Gesellschaft, ix, 1398.

age amount of scheelite in the vein, as a whole, but the high density of the tungstate renders its separation from quartz by mechanical dressing an easy matter, and suggests this as a valuable resource for tungstic acid, the value of which in the form of sodic tungstate, for fire-proofing textile fabrics, is well understood. *Locality.*—Charity Mine, Warren's, Idaho Territory.

My correspondent says: "The miners on this ledge (lode) lately struck a pocket of ore similar to that I send, and in a few evenings pounded out in a hand mortar about one thousand dollars, gold value, from a few candle boxes full of ore. The ore usually pays about twenty dollars per ton."

The affinities of mineral association indicate that cassiterite and wolfram may reasonably be looked for in the future exploration of this interesting vein.

Since writing the above, I have learned of another and much more interesting example of the occurrence of gold in scheelite, from Golden Queen Mine, Lake Co., Colorado. The gold is in minute crystalline granules in the scheelite and fills what appears to have been a geode of scheelite crystals.

New Haven, April 19, 1877.

ART. LII.—*Ninth Report of the British Association Committee, appointed for the purpose of investigating the Rate of Increase of Underground Temperature downward in various Localities of Dry Land, and under Water;* drawn up by Prof. EVERETT, Secretary of the Committee.*

A REMARKABLE series of observations have recently been taken in a boring at Sperenberg, near Berlin. The bore was carried to the depth of 4,052 Rhenish (or 4,172 English) feet, and was entirely in rock salt with the exception of the first 283 feet, which were in gypsum with some anhydrite. The observations were taken under the direction of Herr Eduard Dunker, of Halle-an-der-Saale, and are described by him in a paper occupying thirty-two closely printed quarto pages (206–238) of the Zeitschrift für Berg-Hütten-und-Salinen-Wesen (xx Band, 2 und 3 Lieferung, Berlin, 1872).

The instrument employed for measuring the temperature was the earth-thermometer of Magnus, which gives its indications by the overflowing of mercury, which takes place when the instrument is exposed to a higher temperature than that at which it was set. To take the reading it was immersed in

* The committee consisted of Prof. Everett, Sir W. Thomson, Prof. J. Clerk Maxwell, G. J. Symons, Prof. Ramsay, Prof. A. Geikie, James Glaisher, George Maw, W. Pengelly, Prof. Hull, Prof. Ansted, Prof. Prestwick, Dr. C. Le Neve Foster, Prof. A. S. Herschel, G. A. Lebour, and A. B. Wynne.

water a little colder than the temperature to be measured; the temperature of this water is noted by means of a normal thermometer, and at the same time the number of degrees that are empty in the earth-thermometer is noted. From these data the maximum temperature to which the instrument has been exposed can be deduced, subject to a correction for pressure, which is not very large, because the same pressure acts upon the interior as upon the exterior of the thermometer.

In the following *résumé* (as in the original paper) temperatures are expressed in the Réaumur scale, and depths in Rhenish feet, the Rhenish foot being 1·029722 English foot.

Observations were first taken at intervals not exceeding one hundred feet, from the depth of one hundred feet to that of 4042 feet, the temperature observed at the former depth being 11·0, and at the latter 38·5; but all these observations, though forming in themselves a smooth series, were afterward rejected, on the ground that they were vitiated by circulation of water and consequent convection of heat.

It has often been supposed that though this source of error may affect the middle and upper parts of a bore, it cannot affect the bottom; but the Sperenberg observations seem to prove that no such exemption exists. When the bore had attained a depth of nearly 3,390 feet, with a diameter of twelve inches two lines at the bottom, an advance bore of only six inches diameter was driven seventeen and one-half feet further. A thermometer was then lowered half-way down this advance bore, and a plug was driven into the mouth of this advance bore so as to isolate the water contained in it from the rest of the water above. After twenty-eight hours the plug was drawn and the thermometer showed a temperature of 36·6. On the following day the temperature was observed at the same depth without a plug, and found to be 33·6. Another observation with the plug was then taken, the thermometer (a fresh instrument) being left twenty-four hours in its position. It registered 36·5, and again, without plugging, it gave on the same day 33·9. It thus appears that the effect of convection was to render the temperature in the advance bore 3° R. too low.

Apparatus was then employed for isolating any portion of a bore by means of two plugs at a suitable distance apart with the thermometer between them. This operation was found much more difficult than that above described, but in several instances it gave results which were deemed quite satisfactory; while in other instances the apparatus broke, or the plugging was found imperfect. The deepest of the successful observations by this method was at 2,100 feet, and the shallowest was at 700 feet. The first 444 feet of the bore were lined with iron

tubes, between which the water had the opportunity of circulating even when the innermost tube was plugged, hence the observations taken in this part were rejected.

All the successful observations are given in the third column of the following table, subject to a correction for pressure; and, for the sake of showing the error due to convection in the ordinary mode of observing, the temperatures observed at the same depths when no plugs were used, are given in the second column:

Depth in feet.	Temperature Reaumur.		Difference.
	Without plugging.	With plugging.	
700	16·08	17·06	0·98
900	17·18	18·5	1·32
1,100	19·08	20·8	0·72
1,300	20·38	21·1	0·72
1,500	22·08	22·8	0·72
1,700	22·9	24·1	1·2
1,900	24·8	25·8	1·0
2,100	26·8	27·1	0·3
3,390	34·1	36·15	2·05

These temperatures are not corrected for pressure, but they are corrected for rise of zero in the normal thermometer; and this last circumstance explains the difference of 0·4 between the temperature 36·15 here given and 36·55, which is the mean of the above-mentioned observations at the depth of 3,390 feet.

Another proof of the injurious effect of convection was obtained by comparing the observed temperatures (without plugging) in the first 400 feet of the great bore, designated Bore I, with the temperatures observed at the same depths during the sinking of another bore, designated Bore II, near it; the observations in this latter being always taken at the bottom. The following were the results:

Depth in feet.	Temperature.	
	Bore I.	Bore II.
100	11·0	9·0
200	11·6	10·4
300	12·3	11·5
400	13·6	12·5

The temperature at the depth of one hundred feet in the great bore thus appears to have been raised about 2° R. by convection.

The following is a table of the successful observations, corrected for pressure:

Depth in Rhenish feet.	Temperature Reaumur.
700	17·275
900	18·780
1,100	21·147
1,300	21·510
1,500	23·277
1,700	24·741
1,900	26·504
2,100	28·668
3,390	37·238

Assuming, with Herr Dunker, the mean temperature of the surface to be 7·18, which is the mean annual temperature of the air at Berlin, we have the following increments of temperature with depth:

Depth in Rhenish feet.	Increment of depth.	Increment of temperature.	Increase per 100 feet deg. Reaumur.	Increase per 100 feet deg. Fahr.
0 to 700	700	10·095	1·442	3·24
800 to 900	200	1·505	·752	1·69
700 to 1,100	200	2·367	1·184	2·66
1,100 to 1,300	200	0·363	·182	·41
1,300 to 1,500	200	1·767	·884	1·99
1,500 to 1,700	200	1·464	·732	1·65
1,700 to 1,900	200	1·763	·882	1·98
1,900 to 2,100	200	2·164	1·082	2·43
2,100 to 3,390	1,290	8·570	·664	1·49
0 to 3,390	3,390	30·058	·887	2·00

The mean rate of increase found by comparing the temperatures at the surface and 3,390 feet is exactly 1° Fahr. for 50 Rhenish or 51·5 English feet.

The numbers in the last two columns exhibit upon the whole a diminution with increase of depth; in other words, the temperature increases less rapidly as we go deeper down. As regards the first 700 feet, which exhibit a decidedly more rapid rate than the rest, it must be remembered that nearly half of this distance was in a different material from the rest of the bore, being in gypsum with some anhydrite, while all the rest was in rock salt. Prof. Herschel has found, in recent experiments not yet published, that the conductivity of rock salt is exceedingly high; and theory shows that the rates of increase, in superimposed strata, should be inversely as their conductivities. We may, therefore, fairly attribute the rapid increase in the first 700 feet to the relatively small conductivity of the portion (283 feet) which is not rock salt. The slow rate of increase observed in the long interval between the depths of 2,100 and 3,390 feet is not so easily accounted for; we can only conjecture that this and the other inequalities which the above table presents, for depths exceeding 700 feet, are due to fissures and other inequalities in the rock which have not been put in evidence.

With the view of summing up his results in small compass, Herr Dunker has assumed the empirical formula:

$$t = 7{\cdot}18 + ax + bx^2,$$

t denoting the temperature (Réaumur) at the depth x (Rhenish feet); and has computed the most probable values of a and b, by the method of least squares. He finds

$$a = {\cdot}0129857 \qquad b = -{\cdot}000000125791,$$

the negative sign of b indicating that the increase of temperature becomes slower as the depth increases.

A paper by Prof. Mohr, of Bonn, as represented by an abstract published in *Nature* (vol. xii, p. 545), has attracted attention from the boldness of its reasoning in reference to the Sperenberg observations. Prof. Mohr, however, does not quote the observations themselves, but only the temperatures calculated by the above formula, which he designates, in his original paper (*Neues Jahrbuch für Mineralogie*, etc., 1875, Heft 4), "the results deduced from the observations by the method of least squares." In the abstract in *Nature* they are simply termed "the results of the thermometric investigation of the Sperenberg boring," a designation which is still more misleading.

Attention is called to the circumstance that the successive increments of temperature for successive equal increments of depth, form an exact arithmetical progression, as if this were a remarkable fact of observation, whereas it is merely the result of the particular mode of reduction which was adopted, being a mathematical consequence of the assumed formula $t = 7{\cdot}18 + ax + bx^2$. The method of least squares is not responsible for this formula, but merely serves, after this formula has been assumed for convenience, to give the best values of a and b.

Herr Dunker, in his own paper, lays no stress upon the formula, and gives a caution against extending it to depths much greater than those to which the observations extend. Writing to Prof. Everet under date April, 1876, he requests that in the summary of his results to be given in the present Report, the formula should either be suppressed or accompanied by the statement that its author reserves a different deduction.

The following are the differences between the temperatures computed by the formula and the observed temperatures:

Depth.	Difference (computed *minus* observed).
700	−1·621
900	−0·931
1,100	−1·204
1,300	+0·427
1,500	+0·553
1,700	+0·882
1,900	+0·811
2,100	+0·238
3,390	+0·482

The necessity of adopting some means to prevent the circulation of water in bores, has for some time been forcing itself upon the attention of your committee. Many of the observations taken by their observers have contained such palpable evidence of convection as to render them manifestly useless for the purpose intended; and in the light of the Sperenberg experiment it is difficult to place much reliance on any observations taken in deep bores without plugging. The selection of a suitable form of plug is now occupying the careful attention of your committee.

Herr Dunker's paper gives a very full account of the different kinds of plug employed at Sperenberg.

For stopping the mouth of the advance-bore the plug had a tapering shape, and was of hard wood, strengthened by two iron rings, one at each end. and covered with a layer of tow five lines thick, outside of which was thick and strong linen, nailed above and below to the wood, through a leather strap. It was lowered into its place by means of the iron rods used for boring; and, when in position, was pressed home by a portion of the weight of the rods. The plug carried the thermometer suspended from it. Its extraction was commenced by means of a screw on the beam of the boring machine, in order to avoid a sudden jerk, which might have broken the thermometer. The force which was found necessary for thus starting the plug, as well as the impression observed upon it when withdrawn, showed that it had fitted tight. To insure a good fit, the top of the advance-bore had been brought to a suitable shape, and its inequalities removed, by means of a revolving cutting-tool. Herr Dunker remarks that this plan is adapted to a soft material like rock-salt, but that in ordinary hard rock it would be better to make the bottom of the main bore flat, and to close the advance-bore by an elastic disc pressed over it. The method of observation by advance-bores can only be employed during the sinking of the bore, a time when it is difficult to avoid error arising from the heat generated in boring. The expense of making an advance-bore at each depth at which an observation is required is also an objection to its use.

Another kind of plug devised by Herr Dunker, and largely used in the observations, consisted of a bag of very stout india-rubber (nine millimeters thick) filled with water, and capable of being pressed between two wooden discs, one above and the other below it, so as to make it bulge out in the middle and fit tightly against the sides of the bore. On the suggestion of bore-inspector Zobel, the pressure was applied and removed by means of screwing. Two steel springs fastened to the upper disc, and appearing, in Herr Dunker's diagram, very like the two halves of a circular hoop distorted into an oval by pressing against its

walls, prevented the upper disc from turning, but offered little resistance to its rising or falling. The lower disc, on the contrary, was permitted to turn. Both discs were carried by the iron boring-rods. Rotation of these in one direction screwed the discs nearer together, and rotation in the other direction brought them farther apart. The india-rubber bag could thus be made to swell out and plug the bore when it was at the desired depth, and could be reduced to its original size for raising or lowering. In order to prevent the boring-rods from becoming unscrewed one from another, when rotated backwards, it was necessary to fasten them together by clamps, a rather tedious operation in working at great depths.

In taking observations at other points than the bottom, two of these plugs were employed, one above the other below the thermometer.

In some of the experiments, the apparatus was modified by using linen bags filled with wet clay, instead of india-rubber bags filled with water; and, instead of screwing, direct pressure was employed, the lower disc being supported by rods extending to the bottom of the bore, while the upper disc could be made to bear the whole or a portion of the weight of the rods above it. Some successful observations were obtained with both kinds of bag; but the water-bags were preferred, as returning more easily to their original size when the pressure was removed, and consequently being less liable to injury in extraction. In some observations since taken in another place (Sudenberg), Herr Dunker states (in the private letter above referred to) that india-rubber bags, filled with water, and pressed, not by screwing, but by the weight of the rods, were employed with much satisfaction.

All the methods of plugging employed by Herr Dunker involved the use of the iron rods belonging to the boring apparatus, and therefore would be inapplicable (except at great expense) after the operation of boring is finished and the apparatus removed.

It seems desirable to contrive, if possible, some plug that can be let down and raised by a wire. In the first report of your Committee, it was suggested that two bags of sand, one above and the other below the thermometer, should be used for this purpose. Bags of sand, however, would be liable to rub off pieces from the sides of the bore, and thus to become jammed in drawing up. Mr. Lebour has devised a plug which will be of small diameter during the process of lowering and raising, but can be rendered large and made to fit the bore, when at the proper depth, by letting down upon it a sliding weight suspended by a second wire. Sir W. Thomson suggests that a series of india-rubber disks, at a considerable distance apart, will probably be found effectual.

Mr. Boot has continued his observations in the bore which he is making at Swinderby, near Scarle (Lincoln). It has now been carried to the depth of 2,000 feet, and is in earthy limestone or calcareous shale, of Carboniferous age. Its diameter in the lower part is only $3\frac{1}{8}$ inches. In April last the temperature, 78° F., was observed at 1,950 feet; and more recently 79° F. was observed at 2,000 feet; the water, in each case, having been undisturbed for a month. Supposing these results not to be vitiated by convection, and assuming the mean temperature at the surface to be 50°, we have an increase of 29° in 2,000 feet, which is at the rate of 1° in 69 feet.

Mr. Symons has taken a series of observations at the depth of 1,000 feet in the Kentish Town well, with the view of determining whether the temperature changes. The instrument employed is a very large and delicate Phillips' maximum thermometer. The following is a list of the observations:—

Date of lowering.	Depth indicated. Feet.	Thermometer set at.	Date of raising.	Depth indicated. Feet.	Temperature. Fahr.
1874 ----	1000	64·50	May 8	1007	66·82
" May 8	1000	63·80	July 2	1009	{ (reading lost.
" July 2	1000	63·20	July 28	1005	67·40
" July 28	1000	65·10	Sept. 8	1004	67·51
" Sept. 8	1000	65·80	Sept. 29	1004	67·43
" Sept. 29	1000	65·81	Oct. 30	1006	67·68
" Oct. 30	1000	63·40	Dec. 3	1006	67·52
" Dec. 3	1000	63·80	Jan. 7	1009	67·63
1875 Jan. 7	1000	63·75	Feb. 1	1006	67·56
" Feb. 1	1000	63·90	March 3	1005	67·58
" March 3	1000	63·90	May 3	1006	67·62
" May 3	1000	63·95	June 1	1005	67·49
" June 1	1000	63·00	July 7	1005	67·53
" July 7	1000	63·87	Aug. 3	1004	67·58
" Aug. 3	1000	63·87	Sept. 10	1004	67·68
" Sept. 10	1000	64·00	Oct. 2	1003	67·58
" Oct. 2	1000	63·90	Oct. 19	1004	67·62
" Oct. 19	1000	63·80	Nov. 1	1005	67·62
" Nov. 1	1000	63·70	Dec. 1	Wire broke.	

The "depth indicated" is shown by a measuring wheel or pulley, over which the wire runs by which the thermometer is raised and lowered, as described, with a diagram, in the Report for 1869. The above table shows that there is always some stretching, real or apparent, in the interval between lowering the thermometer and raising it again. Recent observations by means of a fixed mark on the wire, have shown that the change is not, in the main, a permanent elongation, but an alternation of length. It is probably due in part to the greater tension which the wire is under in raising than in lowering, a circumstance which will cause a temporary difference of length varia-

ble with the rapidity of winding up; also in part to the circumstance that the wire is warmer when it has just left the water than when it is about to be let down. Some portion of the irregularity observed may be due to variations of temperature in that part of the well (210 feet) which contains air. The observations, taken as a whole, show that any variations of temperature which occur in this well at the depth of 1,000 feet, are so small as to be comparable with the almost inevitable errors of observation. The observations will be continued at intervals of six months, with additional precautions, and with an excessively slow (specially constructed) non-registering thermometer, in addition to the maximum thermometer hitherto employed.

Through the kindness of the eminent geologist, M. Delesse, of the Ecole Normale at Paris, observations have been obtained from the coal mines of Anzin, in the north of France. They were taken under the direction of M. Marsilly, chief engineer of these mines. Maximum thermometers of the protected Negretti pattern, were inserted in holes bored horizontally to the depth of ·6 or ·7 of a meter in the sides of shafts which were in process of sinking, and in which there was but little circulation of air. A quarter of an hour was allowed to elapse in each case, after the boring of the hole, before the thermometer was inserted, and the hole plugged. Four different shafts were tried. Those designated as Nos. I, II, III, were in the mine Chabaud La Tour; and No. IV was in the mine Renard.

In Shaft I, observations were taken at eight different depths, commencing with the temperature 56½° F. at the depth of 38·5 meters, and ending with 67¾° F. at 200·5 meters.

In Shaft II there were observations at four depths, commencing with 55° at 87·8 m., and ending with 63½° at 185 m.

In Shaft III there were observations at three depths, commencing with 56° at 87·8 m., and ending with 62⅔° at 144 m.

These three shafts, all belonging to the same mine, were very wet, and the temperature of the air in them was 11° or 12° C. (52° or 54° F.).

In Shaft IV, which was very dry and had an air temperature of about 15° C. (59° F.), observations were taken at six depths, commencing with 70¾° F. at 21·2 m., and ending with 84° F. at 134·8 m.

The mean rates of increase deduced from these observations are:—

In Shaft	I,	1° F. in	14·4	m., or in	47·2	feet.
"	II,	"	11·5	m., "	37·7	"
"	III,	"	8·65	m., "	28·4	"
"	IV,	"	8·57	m., "	28·1	"

The observer mentions that in Shaft II there was at the depth of 90 m. a seam of coal, in which heat was generated by oxidation; but no such remark is made with respect to any of the other shafts, although it is obvious that some disturbing cause has rendered the temperatures in shaft IV abnormally high. Possibly the heat generated in boring the holes for the thermometers in this shaft (which was dry) has vitiated the observations, the instruments employed being maximum thermometers. Two of the slow non-registering thermometers mentioned in last year's report have been sent to M. Delesse, to be used for verification.

The slow-action thermometers are constructed on the following plan: The bulb is cylindrical and very strong, and is surrounded by stearine or tallow, which fills up the space between it and a strong glass shield in which the thermometer is inclosed. The shield is not hermetically sealed (not being intended for protection against pressure), but is stopped at the bottom with a cork, so that the thermometer can be taken out and put in again if desired. Stearine and tallow were selected, after trials of several substances, including paraffin-wax, bees' wax, glue, plaster of Paris, pounded glass and cotton wool. The thermometers are inclosed in copper cases lined with india-rubber. When placed, without these cases, in water differing 10° from their own temperature, they take nearly half a minute to alter by one-tenth of a degree.

In concluding this report, your committee desire to express their regret at the losses which they have sustained by the deaths of Professor Phillips, Sir Charles Lyell, and Col. Strange, of whose valuable services they have been deprived within the last three years.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On Iodine Trichloride.*—Christomanos has described, as an excellent lecture experiment, the preparation of pure iodine trichloride by the action of dry chlorine upon dry hydrogen iodide, the latter gas being conducted into the former in excess. The reaction takes place in two stages: $HI+Cl_2=HCl+ICl$ and $ICl+Cl_2=ICl_3$. At the first contact of the two gases, a transient white dense cloud of hydrogen chloride appears in which float innumerable minute gold-yellow flakes of the trichloride. On increasing the flow of gas, reddish-violet fumes appear and the gas frequently ignites, forming an experiment of great splendor. The walls of the vessel are covered with thick, bright lemon-yellow crystals, which are converted into the monochloride by an excess

of hydrogen iodide, but may be preserved in an atmosphere of chlorine. Iodine trichloride fuses at 32° C., is volatilized at 47·5° (though it gives off vapor even at −12°) and is dissociated at 72° into monochloride and chlorine. It is soluble in cold water without decomposition.—*Ber. Berl. Chem. Ges.*, x, 434, March, 1877. G. F. B.

2. *On the Atomic Weight of Selenium.*—PETTERSON and EHMAN have redetermined the atomic weight of selenium by several methods, using for analysis calcium selenate, ammonio-aluminum selenate (an alum) magnesium and silver selenites and silver selenate. The numbers obtained not being sufficiently accordant they tried reducing selenous oxide by means of sulphuric acid and determining the selenium precipitated. In five experiments they deduced the atomic weight of selenium as 79·10, 79·06, 79·08, 79·06, 79·08, the mean of which is 79·08, which may be accepted as the true atomic weight of this metalloid.—*Bull. Soc. Chem.*, II, xxvii, March, 1877. G. F. B.

3. *On Trihydroxylantimonic acid and Antimonyl chloride.*—Analogy points out the existence of an antimonic acid $O{=}Sb{-}\begin{matrix}OH\\OH\\OH\end{matrix}$ and an antimonyl chloride $SbOCl_3$, corresponding respectively to $O{=}P{-}\begin{matrix}OH\\OH\\OH\end{matrix}$ and to $POCl_3$. DAUBRAWA has studied the question of the antimonic acids anew, using Berzelius's method for preparing them, i. e., by dropping antimonic chloride into water. The results of analysis showed that the air-dried precipitate was $H_8Sb_2O_9$; dried over sulphuric acid, it was H_3SbO_4; at 100° C., it became $H_4Sb_2O_7$; at 200°, $HSbO_3$; at 275°, Sb_2O_5; and above 300°, Sb_2O_4. For the preparation of the oxychloride, one part by weight of water was dropped into sixteen parts by weight of antimonic chloride. A yellowish-white distinctly crystalline mass resulted, which was deliquescent in the air, and which fused, boiled and sublimed in a closed tube, dissociating into SbOCl and Cl_2 at higher temperatures. The analysis, which was difficult, gave the formula $SbOCl_3$.—*Liebig's Ann.*, clxxxvi, 110, March, 1877.

G. F. B.

4. *On the Constitution of Cyanamide.*—Of the two rational formulas assigned to cyanamide, $N{\equiv}C{-}NH_2$ and $C\begin{matrix}NH\\NH\end{matrix}$, the existence of di-silver-cyanamide would seem to fix the latter as the more probable constitution. FILETI and SCHIFF have proved, however, that this imide formula is actually not the true one. By acting on di-silver-cyanamide with ethyl iodide, di-ethylcyanamide was obtained, identical in properties and in decomposition products with the di-ethylcyanamide obtained by Cloëz, Cannizzaro and Cahours from cyanogen chloride and diethylamine. As in this latter body the two ethyls are united to the same nitrogen atom—since it came from diethylamine $NH\begin{matrix}C_2H_5\\C_2H_5\end{matrix}$—it

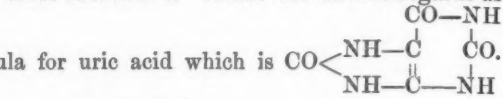
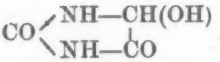
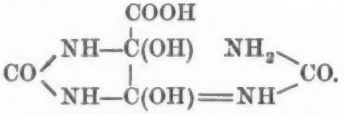
follows that they are in the former; and hence that the rational formula of cyanamide itself is $N\equiv C—NH_2$.—*Ber. Berl. Chem. Ges.*, x, 425, March, 1877. G. F. B.

5. *On Potassium triiodide.*—JOHNSON has succeeded in obtaining potassium triiodide by evaporating a saturated aqueous or alcoholic solution of iodine in potassium iodide, over sulphuric acid. At first dark colored cubes of the iodide colored by iodine appeared; but after some days, lustrous dark blue prismatic crystals were deposited, sometimes two inches long, which afforded the composition of the tri-iodide on analysis. This substance is very deliquescent, becoming a brown liquid on exposure to the air. Its specific gravity at 15° was 3·498. From the fact that its atomic volume is 120·2, by experiment, and 122·2 by calculation from the sum of the atomic volumes $K+I_3$, but only 105·64 from $KI+I_2$, the author infers that it is an atomic and not a molecular compound.—*J. Ch. Soc.*, xxxi, 249, March, 1877. G. F. B.

6. *On Ethyl-mercaptan and its Derivatives.*—In the course of an extended investigation into ethyl-mercaptan and the corresponding mercaptides, CLAESSON has made the curious observation that the group $-\cdot-S.C_2H_5$ of mercaptan is readily exchanged for the haloid constituents of the fatty series, the substitution taking place the more readily the less condensed the carbon nucleus of the compound. Thus by acting on carbon tetrachloride with mercaptan in presence of sodium ethylate, sodium chloride separates, the liquid boils from the heat evolved, and a light yellow oil of 1·01 specific gravity is obtained, which the author calls carbon-tetra-mercaptide, and which has the formula $C(S.C_2H_5)_4$. He has prepared also dicarbonhexamercaptide $C_2(S.C_2H_5)_6$ and dicarbontetramercaptide $C_2(S.C_2H_5)_4$. The author has prepared also ethyl-tetrasulphide $(C_2H_5)_2S_4$ and has studied its properties and reactions.—*J. pr. Ch.*, II, xv, 193, March, 1877. G. F. B.

7. *On the Synthesis of Coumarin.*—TIEMANN and HERZFELD have effected a new synthesis of coumarin. They were led to the method by observing that acetylparacumaric acid was obtained when paroxybenzaldehyde was heated with acetic oxide and sodium acetate, and that benzaldehyde similarly treated yielded cinnamic acid; from which they inferred that salicylaldehyde (orthoxybenzaldehyde) would yield an acetylorthocumaric acid and so coumarin. The reaction readily took place, and the expected acid was produced, and from it also ortho-cumaric and hydro-orthocumaric (melilotic) acid. By simple heating, acetylorthocumaric acid was decomposed into acetic oxide and acid and coumarin which was obtained pure by recrystallization from ether. The authors consider it a lactide-like anhydride with the formula $C_6H_4\begin{matrix}/CH=CH\\ \backslash O——CO\end{matrix}$.—*Ber. Berl. Chem. Ges.*, x, 283, Feb. 1877. G. F. B.

8. *On the Decomposition of Glyoxalyl-urea.*—To test the question whether the substance obtained by protracted boiling of a

solution of uroxanic acid was really glyoxalylurea, as assumed, MEDICUS acted upon it with potassium hydrate, which would split it into urea and glyoxalic acid, and then would decompose the latter into oxalic and acetic acids. The products were examined and proved to be those mentioned. Hence the author regards as proved his formula for uric acid which is  Glyoxalylurea is  and uroxanic acid is 

—*Ber. Berl. Chem. Ges.*, x, 544, April, 1877. G. F. B.

9. *On two new Alkaloids, Strophantine and Ineine.*—HARDY and GALLOIS have examined the poison variously known as inaye, onaye, gombi, etc., extracted in Africa from the seeds of *Strophantus hispidus*, a climbing plant belonging to the Apocyneae. Fraser examined this substance and obtained from it an active extract, to the alkaloid in which he gave the name Strophantine. The authors have succeeded in preparing this body crystallized and have shown that it arrests the action of the heart, the ventricles being in systole. From the tufts about the seeds, they have also obtained a second alkaloid which they call ineine.—*Bull. Soc. Ch.*, II, xxvii, 247, March, 1877. G. F. B.

10. *Occurrence of Copper normally in the Blood of Wild Animals.*—CLOËZ, having to incinerate the blood of a roebuck, killed in the woods, and to analyze the ash, was surprised to detect in it an appreciable quantity of copper. Fearing that it might have entered by accident during his operations, he repeated the analysis on the blood of another buck, taking special precautions. The result was the same, the 530 grams of blood affording three milligrams of copper oxide. It must have existed either in the plants eaten as food by the animal or in the water which he drank.—*Bull. Soc. Ch.*, II, xxvii, 196, March, 1877. G. F. B.

11. *Is Glass impervious to Gases?*—Professor G. QUINCKE has tried to force hydrogen and carbonic acid, by pressures of from 40 to 120 atmospheres, through a glass wall 1·5 mm. in thickness, and to determine by the loss of weight, the quantity of gas that had passed through during seventeen years.

One leg of a V-shaped glass tube was a capillary tube of 200 mms. in length, closed above; the other was a tube contracted in the middle and open above, 150 mms. long, 8 mms. in diameter and 1·5 mms. thick. Into the open leg a drop of quicksilver was put; upon this dilute sulphuric acid was poured; into the upper part some sheet zinc was pushed, which was kept from contact

with the acid by the contraction of the tube; and then the open end was carefully closed by fusion at a glass-blower's lamp. Four tubes thus prepared underwent a double weighing; and then, by inclining the tubes, the sulphuric acid was brought into contact with the zinc. The pressure of the hydrogen was shown by the diminution of volume of the air in the capillary tube, which served for a manometer. Its amount on the first day, in the different tubes, was from 1½ to 10 atmospheres, rose in five months up to 27–54 atmosphere and in 17 years up to 25–126 atmospheres. During this time the tubes were frequently doubly weighed on an excellent balance; and exactly the same weight, within from 0·1 to 0·3 of a milligram, was always found.

Another similar tube, with carbonate of lime and concentrated sulphuric acid, in which the pressure of the carbonic acid gas amounted on the first day to 21 atmospheres, after five months to 34 atmospheres, and after 17 years to 44, showed likewise always the same weight of 14·6361 grams.

Thus according to these experiments, a pressure of from 40 to 100 atmospheres cannot, during a space of seventeen years, force through 1·5 mm. thickness of glass a perceptible quantity of hydrogen or carbonic acid.

While at the commencement the concentrated sulphuric acid wetted the glass sides of the tube, and showed a sharp marginal angle (apparently 0°), gradually in the course of years the angle has become obtuse, and the acid flows in the tube with condensed carbonic acid like quicksilver in a glass tube filled with air.

In the atmosphere of hydrogen the angle at the margin of the dilute sulphuric acid, which at first likewise wetted the sides, has also increased to about 60°.

The glass thus appears to have gradually in the course of years, under the influence of the great pressure, become coated with a thin layer of carbonic acid or hydrogen respectively, which exerts a different attraction from that which glass exerts upon the liquid particles at the margin of the surface. A similar film of gas must have been deposited on the surface of the zinc and obstructed the further chemical action of the acid.—*Pogg. Ann.*, clx, 118; *Phil. Mag.*, iii, 314. E. C. P.

12. *Polarization of the Rainbow.*—M. J. Dechant explains the observations of Schiel and Tyndall that the light of the rainbow could in all cases be extinguished by a Nicols' prism, by Fresnel's formula for the reflection of light. The light falls on the posterior wall of the drop at an angle of 40°, while the angle of total polarization is about 37°. The ratio of the two polarized beams for the red rays is consequently about 24·5 and for the violet rays 34·9. Or, the rainbow is diminished in brightness 25 to 35 fold when the Nicol is rotated 90°. If the index of refraction of the liquid was $\sqrt{2}=1{\cdot}414$ the polarization would be complete. Calculation shows that for oil of cassia the ratio of the red rays is 6·3 and of the violet 2·8. Forming a rainbow by scattering this liquid in sunlight by an atomizer the light cannot be extinguished by a Nicol prism in any position.—*Pogg. Ann.*, clx, 123. E. C. P.

13. *The Radiometer.*—Two views have been offered as to the mode of action of the gas in the radiometer. One attributes the motion to reaction of gas particles getting heated on the vanes, then dancing off; the other to air currents which are directed toward the plate in consequence of heated air rising from it. M. Neesen has endeavored (Pogg. Ann.) to decide between these views. If the second view is correct, he argued, the wall of the vessel, by becoming also heated, must also acquire influence through rise of heated air from it as from the vanes. If the rotation be merely a phenomenon of reaction there is no reason to suppose such an influence of the fixed wall. Now by giving the radiometer an eccentric position within the glass vessel such an influence of the walls should be readily recognized. He describes a number of experiments made in this way, and which he regards as supporting the second view.

In an article contributed to Poggendorff's Annalen, M. Zöllner is led to take the following positions in reference to the radiometer. The explanation of radiometric motions based on the principles of the mechanical theory of gases, make suppositions about the relation of the mean lengths of path of the gas molecules to the dimensions of the vessel which are not realized in fact. This explanation further leaves out of consideration, without sufficient ground, the simultaneous existence of mercury vapors whose molecules have a more then seven times greater mass and a much smaller mean length of path than the molecules of the gases acting according to the mechanical theory of gases. Hence we are not warranted in regarding the radiometric motion discovered by Crookes as an empirical confirmation of the mechanical theory of gases.—*Nature*, xv, 521. E. C. P.

14. *Fluorescence.*—M. E. Lommel discusses the intensity of the light of fluorescence assuming that the quantity of the light which an element of volume of the fluorescent substance can emit is proportional to the quantity of exciting light which is absorbed. He concludes:

(1.) That with an increasing concentration, the intensity of the fluorescent light at first increases up to a certain minimum, and then decreases.

(2.) That the ratio of the light emitted by a superficial layer of given thickness, to the total fluorescent light, increases with the intensity of the absorption, and is as much more feeble as we observe it at a greater distance.

(3.) That the mixture of colors which constitutes fluorescent light changes with the coefficient of absorption of the exciting light, in such a way that the ratio of the intensities approaches more and more that which characterizes the type of the fluorescent spectrum, to the extent that the exciting rays are capable of being absorbed.

(4.) That the fluorescent light, observed at a great distance, is formed of a mixture of colors in which the rays very largely absorbed are in less proportion than when the observation is made at a small distance.

(5.) That by observing the fluorescence by transparency as Lubarsch has done, the proportion of the rays most absorbed is strongly diminished.

(6.) That it is, on the contrary, as great as possible when, the observation being effected on the side of the incident rays, the angle of incidence is very great.

All these conclusions follow naturally from the first proposition and seem to involve only very natural hypotheses.

This theory demands nothing but that the law of Stokes shall be in general correct. It explains, on the contrary, very well the disagreement of the experiments of Lommel, made under conditions theoretically more favorable than those of his opponents.—*Pogg. Ann.*, clx, 75, *Journ. de Phys.*, vi, 126. E. C. P.

15. *Electricity and the Electric Telegraph;* by GEO. B. PRESCOTT. 978 pp. 8vo, with 564 illustrations. New York. 1877. (D. Appleton & Company.)—This volume, by Mr. Prescott, is a most important contribution to scientific literature, honorable alike to the learning and industry of the author. It is a comprehensive review of the application of electrical principles to telegraphy. It discusses with sufficient thoroughness the laws of electrical resistance and measurement; the determination of voltaic constants; the laws of electro-magnetism; of the measurement of electro-motive force; of galvanic polarization and the application of these and other principles to telegraphic circuits and land lines; the phenomena of charge and discharge on land lines and underground lines; submarine cables; electrostatic induction on telegraph lines; the phenomena of charge in submarine cables; coefficient of charge in submarine and land lines; duration of the variable state when the extremity of the line is to earth, and when it is insulated; the time required to produce a signal; the earth a reservoir of electricity, and its electrical resistance. These topics fill nearly half (415 pages) of the volume; the remainder is occupied with a historical notice of experimental telegraphs of the American and European Morse system, and a full notice of all other systems and instruments, illustrated by very beautiful, original wood-cuts. The discoveries and improvements of late years, especially the duplex and quadruplex modes of transmission, and the improved type printing apparatus, which, as the author says, have in America almost revolutionized the telegraph service, are now for the first time illustrated with a completness worthy of the importance of the subject. The harmonic telegraphs or telephones are also described and illustrated. Mr. Prescott's book is prepared with great care, giving to each investigator and inventor just consideration: and while it is full in its technical detail it is also thorough in its scientific accuracy, qualities rarely combined, and worthy of the electrician of one of the most widely extended telegraphic organizations in existence. B. S.

II. Geology and Natural History.

1. *Annual Report on the Geographical Surveys west of the 100th Meridian*, by G. M. Wheeler, 1st Lieut. Eng. U. S. A. 356 pp. 8vo, with many maps and plates. Washington, 1876. —Lieutenant Wheeler's Report, extending to 79 pages, treats of the general results of the expedition. It is followed by appendixes, containing geographical, geological and other information, by Lieut. W. L. Marshall, Lieut. E. Bergland, Lieut. W. L. Carpenter, Lieut. R. Birnie, Jr., Lieut. C. C. Morrison, and Lieut. C. W. Whipple; on meteorology and hypsometry by Lieut. W. L. Marshall; on the geology and mineralogy of Southeastern California—the alkaline lakes, thermal springs and brackish waters of Southern California—the physical and agricultural features and ethnology of Southern California — the meteorology of the Mohave Desert and the geographical distribution of its vegetation, by Dr. O. Loew; on the geology of a portion of Southern California by Prof. J. Marcou; on the geology of the mountain range from La Veta Pass to the head of the Pecos, by A. R. Conkling; economic botany and agriculture of Southern California, by Dr. J. T. Rothrock; on the ornithology and the mammals of portions of California, by H. W. Henshaw; on Orthoptera, by S. H. Scudder; on Coleoptera, by Dr. J. L. LeConte; on the Alpine insect fauna, by Lieut. W. L. Carpenter; on eleven idioms spoken in Southern California, Nevada and on the Colorado River, by A. S. Gatschet.

In the paper by Prof. J. Marcou, the Tertiary beds of Chico Creek, California, are referred to the Eocene Tertiary, instead of Upper Cretaceous, notwithstanding the presence of a few Cretaceous species, as *Ammonites Chicoensis*, and *Baculites Chicoensis*, the species, with these exceptions, having prominently a Tertiary character.

2. *Geological and Geographical Survey of the Territories*, F. V. Hayden, U. S. Geologist in charge.—Bulletin No. 2 of vol. III, recently issued contains a paper on Western Diptera, comprising descriptions and new genera and species, by C. R. Osten-Sacken, occupying 165 pages; a report on Insects collected by P. R. Uhler in 1875, and of Hemiptera collected by A. S. Packard, Jr., by P. R. Uhler, with two plates, extending to 120 pages; and descriptions of the Araneæ collected in 1875, by A. S. Packard, Jr., by T. Thorell, over 50 pages. The memoir by P. R. Uhler contains monographs of the families Cydnidæ and Saldæ.

3. *Contributions from the Laboratory of the University of Missouri;* Report by the Curators of the University of Missouri, to the Governor of the State. Includes a paper by Prof. P. Schweitzer on the various methods of separating and determining Barium, Strontium and Calcium; a paper by C. P. Williams on the composition of Missouri leads (in which the lead constitutes 99·91 to 99·993 per cent, and the silver 0·00029 to 0·00615 per

cent); and analyses of Copper ores, Smithsonite from Dade County, etc.

4. *Organogeny of the Female Flower of Gnetum Gnemon. Della Organogenia dei fiori feminei del Gnetum Gnemon* L., *nota di* O. Beccari. Extr. from *Nuovo Giornale Botanico Italiano*, vol. ix, No. 1. Jan. 1877. With a plate.—Beccari's observations and notes were made at Ternate, in the Moluccas. He was not aware that anything on the subject had then been published in Europe; but in his paper he refers to Griffith as having first asserted that the whole floral apparatus in question belongs to the ovule. Beccari describes the proper female flowers, viz: those of the fertile ament, and distinguishes from them the abortive female flowers which accompany the male ament. The latter have only two envelopes and no embryo-sac; the former have three, and the embryo-sac appears at the time of pollination. The ovule or female flower—whichever it be called—is developed in the axil of an ament-scale, the nature of which is unquestioned; it is sessile, and surrounded by some multicellular "paraphyses." Its body or central part is an ovular nucleus. The question turns upon the nature of the coats. Now these form in succession from without inwards; i. e., the external one is the oldest, and at an early stage is 2-toothed or 2-lobed at apex, as if formed of two confluent parts. Within this appears a second, as a ring or cupule around the base of the nucleus, which in due time it encloses; it shows no indication of a bipartite division or lobing. Soon after this becomes manifest, the third coat originates similarly within the second, advances beyond it and beyond the nucleus, being extended into a styliform tube with a 2-parted apex. This internal and flask-shaped coat is found in the sterile-female flowers of the male ament, but is less prolonged; the external coat equally exists; but the intermediate coat is wanting.

It is taken for granted, on whatever view of the homology, that we have to do with a secondary axis, in the axil of a bract, and that the nucleus is the termination of this axis. According to Griffith the enclosing sacs are coats to the ovule, of which the central body is the nucleus; consequently, this ovule has the anomaly of three integuments. According to Beccari, the nucleus is the whole ovule, viz., an ovule without coats in the manner of that of *Loranthaceæ* and *Santalaceæ*, and the coats are floral organs. A comparison with the quasi-hermaphrodite flowers of *Welwitschia* assures him that the middle and shortest envelope answers to the andrœcium in those flowers, whence it follows that the outer covering is perigonial and homologous with the floral envelope of *Welwitschia*,—only that is tetraphyllous, this is simpler and confluent-diphyllous. It follows also that the innermost sac is carpellary and diphyllous. The gist of the argument in support of this view turns upon these two considerations: 1. *Gnetaceæ* with other Gymnosperms are the lowest type of Phanerogamia; it is most unlikely that such a low type would be provided with such a high type of ovule as one having three coats;

it is more likely that the ovule of *Gnetum* would correspond with the simplified one of *Loranthaceæ*, with which the relationship is not indistinct. 2. The order of evolution of these coverings, i. e., from without inwards or from below upwards, is that of leaves or whorls upon an axis, and of the floral organs in a flower; while ovular coats, as is well known, form in reverse order, the external one latest. The evidence is fairly convincing, and other recent investigations point to the same conclusion.

Some accompanying theoretical deductions drawn by Beccari are not so convincing nor so important. Nor need we share the hesitation with which he uses the name of *Gymnospermæ*. He prefers to continue the term because the ovules, though not destitute of ovary, are destitute of proper ovular coats. But so are not a few angiospermous plants. We should continue it under the original signification, because, admitting the flask-shaped envelop to be homologous with pistil, it is not functionally one; for the pollen reaches and acts directly upon the ovule itself, and not through the intervention of a stigma. Ovules reduced to the nucleus only were not known to Robert Brown, or at least were not considered by him in their present bearing. Being now recognized, the question whether the coat of the seed in *Coniferæ* and *Cycadaceæ* is of carpellary or ovular origin may remain an open one, or may be decided in favor of the former, without essentially derogating from the fitness of the received name for this marked group of orders. Moreover, in flowers so little differentiated as those of *Coniferæ*, the distinction between ovular and carpellary envelopes may be really not cognizable because not yet actual, and so the question may be one of words; while in *Gnetaceæ* an important advance is made, and the ground of a distinction between ovular, carpellary, and perianthial envelopes begins to appear.

If this be so, a vexed question in classification may find a practical settlement. The cultivators of fossil botany, finding that Gymnosperms were far the earliest phænogamous plants, and that no angiospermous Dicotyledons have been detected until long after the appearance of Monocotyledons, almost universally treat the Gymnosperms as a primary division or class of the Vegetable Kingdom. They are seconded by the histologists or organogenists, who naturally make the most of those interesting points of structure which they have brought to view, and which approximate the Gymnosperms to the Vascular Cryptogams. But the question whether Gymnosperms are a part—the earliest and simplest part—of the great class of Dicotyledons, or whether they constitute an independent class or primary group, must be determined by broad and general considerations of the whole structure. Now the transition from *Gnetaceæ* to Angiospermous Dicotyledons is obvious and apparently real. If no transitions are extant between the Gymnosperms and Cryptogams, and if—as is clear—the former are truly Dicotyledonous and exogenous in structure, and have greater affinity with the Angiospermous Dicotyledons than

they have with the Monocotyledons, then the taxonomist would appear to have good grounds for concluding that the proper division of the vegetable kingdom is, first into *Phanerogamia* and *Cryptogamia;* then the former into *Monocotyledons* and *Dicotyledons*, then these last into *Gymnosperms* and *Angiosperms.* A. G.

5. *The Paleontological Origin of those trees and shrubs indigenous to the south of France, which are sensitive to Frost in cold winters* (*Sur l' Origine paleontologique, etc.*); by CHARLES MARTINS, 1877, 4to. Extr. from Mém. Acad. Montpellier, tom. ix, pp. 87–122.—Prof. Martins gives a list of 19 of those woody plants which are killed down to the ground by the cold which is occasionally experienced at Montpellier, or on the Mediterranean coast from Peripignan to Mentone, but which spring again from the base and flourish until another exceptional frost occurs. The list ranked in the order of tenderness, begins with the Carob-tree, *Ceratonia siliqua*, and ends with *Vitis vinifera.* With very few exceptions, these plants are solitary representatives in Europe of their genus and tribe; they are represented in a fossil state in the Tertiary formations of S. Europe and also of middle Europe, either by identical species, or by others so similar that they are regarded by paleontologists as equally ancestral. Wherefore, upon a full discussion of the details, it is concluded that these are remains of the Tertiary flora of Europe, which have thus far resisted with success the rigors of a present climate to which they are not completely adapted. A. G.

6. ALEXANDER BRAUN.—We announce with sorrow the death of this excellent botanist, which took place in Berlin, on the 29th of March, after a short illness. Systematic botanists of the first class are everywhere rare, and especially in Germany, where they have gone out of fashion, all attention being turned to histology and the like. In Braun's earlier days there was a goodly array of systematic botanists in Germany: at his decease there are very few of mark, although signs of revival are apparent. Alexander Braun was born at Ratisbon, May 10, 1805, but was brought up at Carlsruhe, where his father became a trusted officer in the post office department. Fifty years ago, there was a knot of closely-allied students at the University of Heidelberg, consisting of Braun, Carl Schimper, Agassiz, and Engelmann. Two of them were transferred to our own soil; the latter is now the sole survivor. Three of them went soon to Munich, where Oken, Schelling, Dollinger, and Martius were teaching; but Braun, Agassiz, and Engelmann met again as fellow-students at Paris in 1832. The first two became allied afterwards by the marriage of Agassiz with Braun's sister. About the time that Dr. Engelmann came to the United States, Braun was made professor of Botany and Zoology in the polytechnic school of Carlsruhe. In 1846 he took the chair of Botany in the University of Freiburg in the Briesgau; was transferred to Giessen in 1850; but in the spring of 1851 was called to Berlin, as the successor to Link and Kunth, taking charge of the Botanic Garden as well as of the professor-

ship. Although he had nearly reached the age of 72, and felt the full weight of his years, yet he was assiduously attending to his official duties when he was suddenly prostrated by acute disease of the chest, terminating fatally. Although learned in almost every department of his science, his *forte*, like that of Agassiz, was morphology, and his systematic work mainly among the higher and some of the lower Cryptogamia, *Marsilia*, *Isoetes*, *Chara*, etc. Although his communications to the scientific journals began as early as the year 1822, when he was only 17 years old, his first contribution to science of much extent, and of high and permanent value, was his memoir on the arrangement of the scales of pine-cones, etc., published in 1830. With this publication began the present knowledge of phyllotaxis. It is well understood that the first steps were taken by his fellow-student, Carl Schimper, and that the early investigations were pursued in common by the two. But Schimper published nothing, or next to nothing, either then or since, although he lived until the year 1867. His name in connexion with the subject is preserved by the favorable mention of his companions and contemporaries; but Braun's treatise was timely and fruitful, and became classical. Braun's ability for the philosophical treatment of vegetable morphology and development was manifested in his next large paper, viz: in his memoir entitled Rejuvenescence in Nature, especially in the life and development of plants. This was first published at Freiburg in 1849–50, and again at Leipsic in 1851, and an English translation of it was published by the Ray Society in 1853. Of a similar character, and marked with equal acuteness, is his essay on The Vegetable Individual in its relation to Species, etc., published in 1853 at Berlin, and which, in a translation by a pupil of mine, was mainly reproduced in this journal (May and Sept., 1855). He reaches the conclusion—which would now be more confidently expressed—"that the individual appears in its full import only in the higher steps of the series of created beings."

In his systematical work, Braun was exceedingly laborious, persevering, and conscientious. When we add that throughout the riper and what should have been the most productive years of his life, he was overtasked with official duties and cares, we shall not wonder that much which he hoped to accomplish is left undone. His work upon *Marsilia*, *Pilularia*, and *Isoetes* may be essentially complete. But his prolonged studies of *Chara*, which began forty years ago, and the completion of which would have crowned his career, have probably not been finished, or brought into such form that the results may be fully secured.

His influence as a teacher is said to have been great; as an investigator he stood in the first rank among the botanists of our time; as a man his simple, earnest, and transparently truthful character won the admiration and love of all who knew him.

A. G.

7. *Notice of Recent Works on Vegetable Paleontology;* by L. LESQUEREUX.—Professor O. HEER has added a fourth volume to his Arctic Flora, a noble supplement to a great and justly celebrated work. The volume contains representatives of the Spitzbergen flora at four epochs of the Carboniferous, the Jurassic, the Cretaceous and the Miocene. It thus affords materials for comparison with those of the same far distant geological times whose floras have become more intimately known from their representatives in the middle part of Europe, and partly, or for the Miocene and the Carboniferous, from those studied from the American continent also.

The first part describes species of the upper Carboniferous of Bell Sound. Even if the stage of the formation, where the plants have been found, is not positively ascertained, the character of these plants point to a more recent deposit than that of Bear Island, described in the second volume of the Arctic Flora. For, out of twenty-six species, it has seven small ferns, *Sphenopteris* and *Adiantites*, a single *Lepidodendron*, one *Stigmaria*, three *Sphenophyllum*, and three *Cordaites*, with a new genus, *Rhynchogonium*, established for beaked fruits clearly related to *Nœggerathia* or *Cordaites*, by its linear ribbon-like striated leaves. One species more than any other, shows the relation of this group of plants to the upper Carboniferous; a *Walchia*, a genus mostly Permian. For there is as yet positive evidence of its presence in the Carboniferous by one species only, in the upper part of the formation in Europe, and none in that of this continent. This small group of Carboniferous plants of Spitzbergen differs in its character from that of Bear Island, which, referable to the Subcarboniferous (the Culm or Mountain Limestone,) separated by Heer in two different periods, has *Calamites radiatus* (*Bornia*), species of *Cardiopteris*, one *Paleopteris*, one *Sphenopteris*, the only plant of a type analogous to the species of Spitzbergen, and the balance, large *Lycopodites*, of the genera *Lepidodendron*, *Knorria*, *Cyclostigma*, *Halonia*, etc., all related to types considered in Europe as representatives of the flora of the Culm and in America of the Subcarboniferous. *Cardiopteris* and *Paleopteris* are of a still more ancient type rarely found in the Subcarboniferous of this country. This division is important as recording for the plants of the Coal in the Arctic regions a distribution analogous to that which is now recognized in the temperate zone of both Europe and America.

The Jurassic flora of Cape Boheman in Spitzbergen, thirty-two species, is supplemented in the same volume by that of the Amour and of Eastern Siberia, eighty-three species, whose analogy with the Jurassic flora of England is marked by sixteen species, and by seven with that of Boheman; a relation showing, as in the Carboniferous flora, an analogous distribution of species in the Arctic and the temperate zones during the Jurassic times. In his introduction, Heer rightly remarks on the importance of the characters of those northern Jurassic types, as putting aside the assertion that as long as we knew nothing of the old floras of the Polar regions, we could not really argue upon the analogy of the floras

over a whole continent at a given period of time. From the Carboniferous to the Miocene inclusive the same relation is remarked in the plants of the continent of Europe, and so far as the floras are known, in that of America from the 40th to the 80th degrees of latitude. In tracing analogous or identical types from the Carboniferous flora of Portugal, from the Jurassic of England, from the Miocene of Italy, to those of the same formations of Greenland and Spitzbergen, we can derive reliable conclusions in regard to the characters of the vegetation of these epochs over the whole northern hemisphere, if not over the whole world.

Of the descriptions of the species of these Jurassic plants in the fourth volume of the Arctic Flora, of the perfection of the figures with which they are illustrated, it is not possible to speak with too high commendation. The more common types, that of the *Ginkgo*, of the *Baiera*, while especially represented by numerous figures, are restored in their predominant characters. A new genus, *Czkanowskia*, related to *Trichopitys* of Saporta, has two species exposed in their different organs by two full plates of figures. *Baiera longifolia*, *Ginkgo sybirica* are represented with leaves, flowers and fruits, the fructification of the *Ginkgo* being very similar to the small *Cardiocarpus* of the Coal. One of its species, *Podozamites lanceolatus* is, in its leaves, remarkably similar to the one described as *Pterophyllum? Haydenii* in the Cretaceous flora of Nebraska.

A few vegetable remains of the Cretaceous of Cape Staratschin, already mostly known as described in a former volume, constitute the third part, a very short one, of three pages and half a plate of figures.

The fourth part is a supplementary description of seventy-one Miocene species of three different localities of Spitzbergen: Cape Lyell, the Scott Glacier, and Cape Heer. The relation between the plants of these localities is indicated by a comparatively large number of species, thirty-four being common to Cape Lyell and Scott Glacier, and seven found also at Cape Heer, which has only fifteen species for its flora. Considering the whole group altogether we find it related to the Greenland Miocene flora by twenty species, to that of Alaska and Sitka by eleven, five of which are also present in Greenland; to that of Europe by twenty-one, and to that of the Upper Lignitic of the Rocky Mountains, the Carbon and the Green River groups, by seven. This relation is more than sufficiently close to prove contemporaniety of the formation, even with the North American Miocene, though the number of identical species may appear small. It is even more evident than with the European Miocene; for the Tertiary flora of this last continent is now known by more than four thousand species, while, including even the Lower Lignitic Eocene, we know scarcely as many hundred from the North American Tertiary formations. Therefore the conclusions taken in considering the distribution of the Carboniferous and Jurassic floras are valid in the same degree for the Miocene. The essential types of the vegetation of this epoch are the same over the

whole northern hemisphere, modified, however, by a difference already remarked in the climatic circumstances of regions under distant latitudes. For the Spitzbergen Miocene flora has no species of Ficus, nor any kind of Laurineæ. It has, however, two Magnolias representing, probably, the highest degree of temperature of the Arctic of that epoch, equivalent to that at the present time of the middle States of North America, South Pennsylvania, and Ohio, for example, while the Tertiary flora of the Western Territories, Colorado, and Wyoming, has from the Eocene to the upper group, the Pliocene of California, representatives of species now inhabiting the shores Gulf of Mexico, and extending even as far south as Cuba and New Mexico.

The description of eight species from Disco, with two plates of illustrations, closes this fourth part of the Arctic Flora. The species are mostly referable to Jurassic types.

The volume under consideration has been prepared from specimens collected by the Swedish Polar expeditions in the years 1872 and 1873. These specimens were examined by Heer and described in 1874; but the publication of the work then ready has been delayed by circumstances independent of the author's control. In the meanwhile, the celebrated Professor has undertaken the preparation of the floras of the different geological formations of Switzerland, beginning with that of the Carboniferous, just published.* The second part, that of the Jurassic, is complete also, and ready to appear. The Carboniferous flora represents the plants of the Anthracite of Vallais, a formation of very limited extent. The general character of these plants is that of the Upper Carboniferous, marked mostly, at least, by a preponderance of ferns: *Sphenopteris*, *Neuropteris*, *Cyatheites*, *Pecopteris*, etc.; the *Calamariæ* and the *Cordaites*, which the author considers as Conifers. A number of species of *Lepidodendron* and *Sigillaria* are still in this group; and as it has also *Sphenopteris latifolia*, *trifoliata*, etc., the large *Alethopteris*, *A. nervosa*, *Serlii*, *muricata*, *Pluckneti*, which in our Coal measures are more abundant in the middle group or above the Millstone grit, the general facies of this Anthracite flora, judging from the American point of view, is rather that of the middle than of the upper Carboniferous. But it has one species of *Walchia* which, together with some ferns, *Pecopteris arborescens*, *Neuropteris Loschii*, etc., are mostly abundant in the upper Coal measures, even passing to the Permian, and therefore the conclusions of the author on the age of the formation have part of the evidence in their favor. A peculiar trait, marking the vegetable remains of the Anthracite of Switzerland, is the presence, upon the surface of the leaflets of ferns especially, of a coating of very hard carbonaceous matter, nearly transformed into graphite, which generally obliterates the nervation, and the deformation of these pinnules which are often drawn and elongated on one side of the pinnæ while they are con-

* Flora fossilis Helvetiæ. Erste Lieferung. Die Steinkohlenflora. Zurich. 1876. T. Wurster & Co.

tracted or shortened and enlarged on the other. The fossil plants of Rhode Island present exactly the same appearance, the dimorphism of the ferns being there still far more distinctly marked than in the species figured by Heer, and the nervation rendered quite as obscure by the superposition of the coating of hard coaly matter, apparently a transformation of the epidermis by heat. I have tried to explain the deformation of the ferns, as a result of a slow movement of upheaval in successive undulations of the strata as it is remarked along the beach of Newport; a movement which, while the plants were still in a soft state, caused an extension or traction to one side, and therefore this peculiar deformation of the leaflets when placed on one side of the line of the force of upheaval. Another remarkable coincidence is, that *Neuropteris alpina* St., a very rare species, found in Europe only in the anthracite of Switzerland, is found also at Newport, and has not been until recently seen anywhere else in the American Coal-measures. The Carboniferous flora of Heer contains descriptions and figures of one hundred species, seventeen of which are considered as new by the author.

Professor Heer has, it seems, opportunity to study plants of most of the geological formations of Europe, and indeed his profound knowledge of fossil botany seems to render every work related to this science an easy task for him. He has also recently published a pamphlet on some Permian plants of Hungary,* and there describes one species of *Baiera*, one of *Ulmannia*, two *Voltzia*, one *Schizolepis*, with six *Carpolithes;* these all new species. The work exposed in this short review, and which scarcely represents two years of the labors of the celebrated professor, would make an honorable record for a whole scientific life.

Another European paleontologist, Count Saporta of Aix, enjoying a degree of celebrity as high and as well merited as that of Professor Heer, and equally versed in the study of the fossil plants of the different formations of Europe, has lately with the assistance of his friend, Dr. Marion, published the Pliocene flora of Maximieux. By the exposition of transitional forms traced to living species of the same country, this flora is like a link between the vegetation of the Miocene and that of the present time, and thus affords evidence on the succession and modification of vegetable types which has not been obtained elsewhere until now. The work† describes thirty-two species splendidly illustrated, most of the figures having in contraposition a representation of the living species to which they are compared as identical or intimately related. The exhibition of this relation gives subjects for consideration as instructive to American as to European botanists, if not more so. We find in it a species of *Torreya*, a genus out of Europe now, but present in California; *Platanus aceroides* var. *cuneifolia* intimately related to, if not identical

* Ueber Permische Pflanzen von Fünfkirchen in Ungarn, von Dr. Oswald Heer. Budapert, (1876.)

† Recherches sur les végétaux fossiles de Maximieux par le Comte G. de Saporta et le Dr. A. F. Marion. Lyon, etc. (1876.)

with the North American *Platanus occidentalis*, var. *acerifolia*. *Liquidambar Europeum*, a Miocene species without relation now in the European flora, but reproduced still in that of our continent; in the family of the Laurineæ the group of Maximieux has *Persea Carolinensis;* in the *Magnolia*, a species similar to the American *M. grandifolia;* and still more, one *Lyriodendron*, a *Tilia* represented now by our *T. pubescens*, and an *Ilex* related to *I. Cassine*. Thus, nine species of this group of thirty-two Pliocene European species belong by identity or close affinity to the present North American flora, while ten only are European; the others being related by nine species to the flora of the Canary Islands and by four to Asiatic types.

Besides its intrinsic value, Saporta's work has a peculiar interest for the paleontology of this continent, where only, after Maximieux, materials pertaining to a pliocene vegetation have been obtained; first from the Chalk Bluffs of the Mississippi, below the mouth of the Ohio, and later and more abundantly from the gold-bearing gravel beds of Nevada County, California. From this last formation fifty species are known, and ten from the former; and of these sixty species, forty-six are American, either identical with or closely allied to some species of our present flora. Of the number, six are already represented in the Lignitic flora of the Rocky Mountains, a fact proving the long continuance of vegetable types on this continent; for the American types of the Pliocene of Maximieux, *Platanus*, *Liquidambar*, *Persea*, *Magnolia*, *Lyriodendron*, etc., are all already present in the American Miocene as in that of Europe, and therefore have not come to us by migration. This subject, however, of transformation and migration of vegetable types is examined with some more details in a memoir on the Pliocene flora of California, now in the way of publication as a new volume on the Geology of California, by Professor J. D. Whitney. It is, therefore, more appropriate to postpone a consideration of it for the present.

8. *Preliminary Note on the Development of Organisms in Organic Infusions;* by JOHN TYNDALL, F.R.S. Received January 18, 1877.—I beg leave to submit to the Royal Society a brief preliminary note of the results obtained in the further prosecution of my researches "On the Optical Deportment of the Atmosphere, with reference to Putrefaction and Infection."

The very remarkable experiments of Dr. Roberts, of Manchester, which have been confirmed by Professor Cohn, of Breslau, have been both verified and contradicted by my recent researches. In some cases alkalized hay-infusions have been completely sterilized by boiling for five minutes, in other cases they have withstood the boiling temperature for a far longer period.

Pursuing with scrupulous exactness the method of experiment devised by Dr. Roberts, and in part followed by Professor Cohn, I have found in other infusions than hay an enormous resistance to sterilization. A single conspicuous example will serve as an illustration. Cucumber-infusion has been subjected, for intervals

varying from five minutes to five hours and a half, to the boiling temperature without losing its power of developing life. Two days' exposure to a temperature of 90° Fahr., subsequent to this treatment, sufficed to develop in it swarms of *Bacteria*.

The infusion which thus withstood, in one of Dr. Roberts's "plugged bulbs," the temperature of boiling water for 330 minutes, was completely sterilized in three minutes by boiling it in a small flask with a narrow neck, and hermetically sealing the flask during ebullition. In the case of the "plugged bulbs" the observed resistance was due, not to the germs of the infusion, but to those diffused in the air above it.

I have also pursued my experiments with closed chambers, from which the floating matter was removed by self-subsidence. With certain new infusions introduced into this inquiry failure after failure occurred, two or three days generally sufficing to fill the boiled and protected liquids with Bacterial life. The vegetable infusions usually became turbid throughout; but a characteristic feature of the life developed in all infusions during the last three months was the formation upon their surfaces of a thick and deeply pitted fatty scum. Precautions far greater than those found successful a year ago failed to protect these infusions from contamination.

I resorted to the mode of calcination by an incandescent platinum wire, applied with such uniform success in my last inquiry. The wire was brought close to its point of fusion, the period of incandescence was doubled, and extraordinary care was taken to ward off infection by a ring of cotton-wool. The care proved nugatory; for, in despite of it, swarming life appeared in the infusions afterwards.

I tried to reproduce the results with animal infusions obtained with such ease and certainty a year ago. Some of these old infusions, highly concentrated by evaporation, remain with me to the present hour; they are as clear as distilled water. But in my recent experiments, where the care bestowed far exceeded that found necessary in my last inquiry, the animal infusions, like the vegetable ones, fell, for the most part, into putrefaction.

With hermetically sealed flasks, properly boiled and sealed with due care (I would emphasize this condition), there was no difficulty in sterilizing any of the animal infusions.

By the scrupulous removal of every possible source of contamination I was able finally to maintain some of the most refractory of the liquids operated on perfectly pellucid, in closed chambers from which the floating dust had disappeared by self-subsidence.

It is to be noted that the earliest experiments of this inquiry were quite in harmony with all the results of the former one, and that it was only as time advanced that the singular discordance between recent and former results showed itself in any marked degree. What was the cause of this discordance?

The question is to be answered by reference to the experiments with hay-infusions, which were begun early and were multiplied

and varied later on. By practice such a mastery over these infusions was at length attained that, though the same method of experiment was undeviatingly pursued, I could contradict or corroborate, at will, the observations of Dr. Roberts and Professor Cohn.

On analyzing these apparently irreconcilable results, it was found that, in almost every case where five minutes' boiling sufficed to sterilize alkalized hay-infusion, the hay employed was mown in 1876, while in almost every case where the greater resistance to sterilization was shown, the hay was mown either in 1875 or some previous year. The hay found most difficult to sterilize was from Colchester, and it was five years old.

To the drying and hardening of the germs of the old hay by time I ascribe this singular result.

An experiment on artificially dried peas, as compared with the same peas undried, is not without instruction. After boiling for an hour or so, the undried peas become tasteless, while the dried ones retained a considerable amount of flavor. After a couple of hours' boiling the undried peas rendered the water in which they were immersed thickly turbid, the liquid surrounding the dried peas remaining at the same time perfectly clear. The dried peas were rendered soft, but many of the green peas were reduced by two hours' boiling to a mere pulp, the mixture of which with the water rendered it muddy.

The comparative tastelessness of the undried peas proved that their juices, which are an essential factor of their individuality, and probably also of their power of germination, had diffused into the surrounding water. On the other hand, the clearness of the water which embraced the dried peas indicated a restriction of the exchange of matter between the peas and the medium in which they were immersed. The experiment threw light upon the fact that even with four or five hours' digesting, it was impossible to make the specific gravity of the samples of my oldest hay sensibly greater than that of water. The dryness and induration of the old hay thus indicated being shared by the germs attached to the hay, endowed them, I doubt not, with their greater power of resistance.

Experiments have also been made with new hay dried artificially at temperatures varying from 140° to 185° Fahr., an account of which shall be communicated in due time to the Royal Society.

The different samples of hay employed in this investigation were introduced in succession into the laboratory of the Royal Institution, and they ended by rendering the atmosphere of the place so virulently infective that precautions which, under ordinary circumstances, were more than sufficient to secure perfect immunity from external contamination, were found utterly ineffectual.

Thanks to the friendly action of the President of the Royal Society, I was enabled to escape from this atmosphere to a purer

air. I had a series of tin chambers constructed, which were not permitted to enter the Royal Institution at all, but were taken straight from the tinman to Kew Gardens. They were mounted in the excellent laboratory recently erected there by the munificence of Mr. Jodrell. In this new position the insuperable difficulties encountered in London disappeared, and the experiments followed the course of those described in my last investigation. Two of the chambers gave way; but on being scrutinized they were found leaky. Five sound chambers, on the contrary, remained perfectly intact, and they embraced the particular substances which had given me so much trouble in London. Infusions exposed to the common air at Kew became rapidly rotten.

A fuller account of these researches shall soon be submitted to the Royal Society. In prosecuting them thus far I have been very ably assisted by Mr. Cottrell and his junior colleague Mr. Frank Valter.—*Proc. Roy. Soc.*, vol. xxv, No. 177.

9. *On Heat as a Germicide when discontinuously applied;* by JOHN TYNDALL, F.R.S. Received Feb. 14, 1877. (Letter addressed to T. H. Huxley, Esq., Sec. R. Soc., and dated Feb. 14, 1877.)—In my "Preliminary Note," communicated to the Royal Society on the 18th of January, various infusions were referred to as manifesting an astonishing resistance to sterilization by heat. This resistance was traced to its source; and I have been since informed that you were good enough to express at the time a very favorable opinion as to the significance and value of the results indicated.

It will, I think, now interest you to learn that the most obstinate of the infusions referred to in the "Note" have been since rendered tractable by the application of very simple means. Following up the plain suggestions of the germ theory, I have been able, even in the midst of a virulently infective atmosphere, to sterilize all the infusions by a temperature lower than that of boiling water.

It is known that the prolonged application of a low temperature is often equivalent to the brief application of a higher one: and you may therefore be disposed to conclude that in the experiments here referred to I have substituted time for intensity. This, however, is not the case. The result depends solely upon the manner in which the heat is applied. For example, I boil an infusion for fifteen minutes, expose it to a temperature of 90° Fahr., and find it twenty-four hours afterward swarming with life. I submit a second sample of the same infusion to a temperature lower than that of boiling water for five minutes, and it is rendered permanently barren.

The secret of success here is an open one. I have already referred to the period of latency which precedes the clouding of infusions with visible *Bacteria*. During this period the germs are being prepared for their emergence into the finished organism. They reach the end of this period of preparation successively—the period of latency of any germ depending upon its condition as regards dryness and induration. This, then, is my mode of pro-

ceeding: before the latent period of any of the germs has been completed (say a few hours after the preparation of the infusion), I subject for a brief interval to a temperature which may be under that of boiling water. Such softened and vivified germs as are on the point of passing into active life are thereby killed; others not yet softened remain intact. I repeat this process well within the interval necessary for the most advanced of those others to finish their period of latency. The number of undestroyed germs is further diminished by this second heating. After a number of repetitions, which varies with the character of the germs, the infusion, however obstinate, is completely sterilized.

The periods of heating need not exceed a fraction of a minute in duration. Sum them up in the case of an infusion which they have perfectly sterilized; they amount altogether to, say, five minutes. Boil another sample of the same infusion continuously for fifteen or even sixty minutes, you fail to sterilize it, although the temperature is higher and its time of application more than tenfold that which, discontinuously applied, infallibly produces barrenness.

In a few weeks I hope to bring this entire subject under the notice of the Royal Society.—*Proc. Roy. Soc.*, Feb. 1, 1877, vol. xxv, No. 178, p. 569.

III. Miscellaneous Scientific Intelligence.

1. *National Academy of Sciences.*—At the session of the National Academy, held in Washington, April 17–20, 1877, the following new members were elected: Elliott Coues, U. S. A., Washington, D. C.; John W. Draper, New York; Henry Draper, New York; S. H. Scudder, Cambridge, Mass.; C. S. Peirce, Cambridge, Mass.

The following is a list of the papers read at the meeting:

On a new measuring instrument: the vernier microscope; by A. M. Mayer.

On the laws ruling the vibration of tuning-forks; by A. M. Mayer.

On the determination of the coefficient of expansion of solids; by A. M. Mayer.

On some remarkable facts in the distribution of magnetism in very long (5 ft. $\frac{1}{2}$ in.) magnets; by A. M. Mayer.

On systematic errors in star declinations; by E. C. Pickering and W. A. Rogers.

Micrometer-level and topographical camera; by E. C. Pickering.

On the young stages of some osseous fishes; by Alex. Agassiz.

The results of deep-sea dredgings; by Alex. Agassiz.

On critical periods in the history of the earth, and their relations to evolution, and on the Quaternary as such a period; by Joseph LeConte.

On the structure of the crystalline lens, and its relation to periscopism; by Joseph LeConte.

On the progressive motion of storms; by William Ferrel.

Contribution to Meteorology; by Elias Loomis. (7th paper.)

Improved method of obtaining metallic spectra; by G. F. Barker.

On the effect produced by mixing white with colored light; by O. N. Rood.

On Newton's use of the term *indigo* with reference to the color of the spectrum; by O. N. Rood.

Internal structure of the earth as affecting the phenomena of precession and nutation: supplementary to article under this head read before the Academy, and published in vol. xiii, Smithsonian Contributions; by J. G. Barnard.

A proposed new method in solar spectrum analysis; by S. P. LANGLEY.
On some researches in the theory of invarients; by J. J. SYLVESTER.
Peculiarities noticed in the oxidation of aromatic sulpho-acids; by IRA REMSEN.
On complex inorganic acids; by WOLCOTT GIBBS.
Description of a detached gravity escapement; by C. A. YOUNG.
Remarks on the apparent secular acceleration of the mean motion of the moon, as derived from the observations of the ancient eclipses; by SIMON NEWCOMB.
Researches in linear theory; by GEO. W. HILL.
On the structure of the "Henry" Mountains; by G. K. GILBERT.
On the public domain; by J. M. POWELL.
Remarks on some artesian wells, along the line of the Union Pacific Railroad in Wyoming Territory; by F. V. HAYDEN.

2. *Excrementitious deposits in the Rocky Mountain region.*—A paper on these deposits, in Nevada, by H. W. HENSHAW (Proc. Acad. Sci. Philad., 1877, 39) sustains the view of Prof. Cope, that they were made by the big-eared rats, a species of *Neotoma*, and probably *N. cinerea*. They consist of vegetable matter, as shown by the chemical analysis of Dr. Lœw, in the volume on Zoology, of Lieutenant Wheeler's expedition. Part have a bitumen-like look, and others vary from this to pill-like excrements. In a crevice of the rocks one deposit had a depth of two feet, and contained also small twigs and some birds' feathers. "The mass was evidently the accumulation of years, and had served as a nest. Throughout was a large amount of hard droppings from which the urine had passed, and whose nature was unmistakable. The urine, charged with a certain amount of excrementitious matters, had filtered through to form the singular deposits." Water or the urine has carried the portions it could dissolve down the faces of walls and deposited it on shelves where no animals without wings could reach, and sometimes on the roofs of cavities. All the regions where these deposits occur are inhabited by the *Neotoma*, which is eminently a vegetarian.

3. *Fourth Annual Report of the New Jersey State Board of Agriculture for the year* 1876. 196 pp. 8vo. Trenton, N. J.—Besides details of economical importance, this Report contains an account of the Green sand beds of the State by Prof. G. H. Cook (Secretary of the Board) with sections, and detailed descriptions, and many analyses of the green sand and clayey marls. All belong to the Cretaceous, excepting the upper layer of the Upper Green sand, which is Eocene Tertiary. The value of the green sand marl, in agriculture, is treated of at length. There is also an account of the Miocene Tertiary marls of the State.

4. *Primer of Chemistry, including analysis;* by A. VACHER. 108 pp. 12mo. London, 1877. (Churchill).—The author states that this little book represents ten years' experience in teaching the rudiments of chemistry and analysis. The term *unit* replaces *atom* and *molecule*, and *antimetal* serves in place of *radical*, and the student is left to guess at the meaning of atomic weight and equivalent, these terms not being found in the book. The science is compressed into a small compass, but the condensation passes the limit of clearness. B. S.

5. *Correction.*—Comet b, 1877, was discovered by WINNECKE.

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